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SOLAR PHYSICS

Edinburgi, in Academiâ Jacobi Sexti
Scotorum Regis,

Mensis Aprilis die IX MDCCCLXXX

Classis Primi

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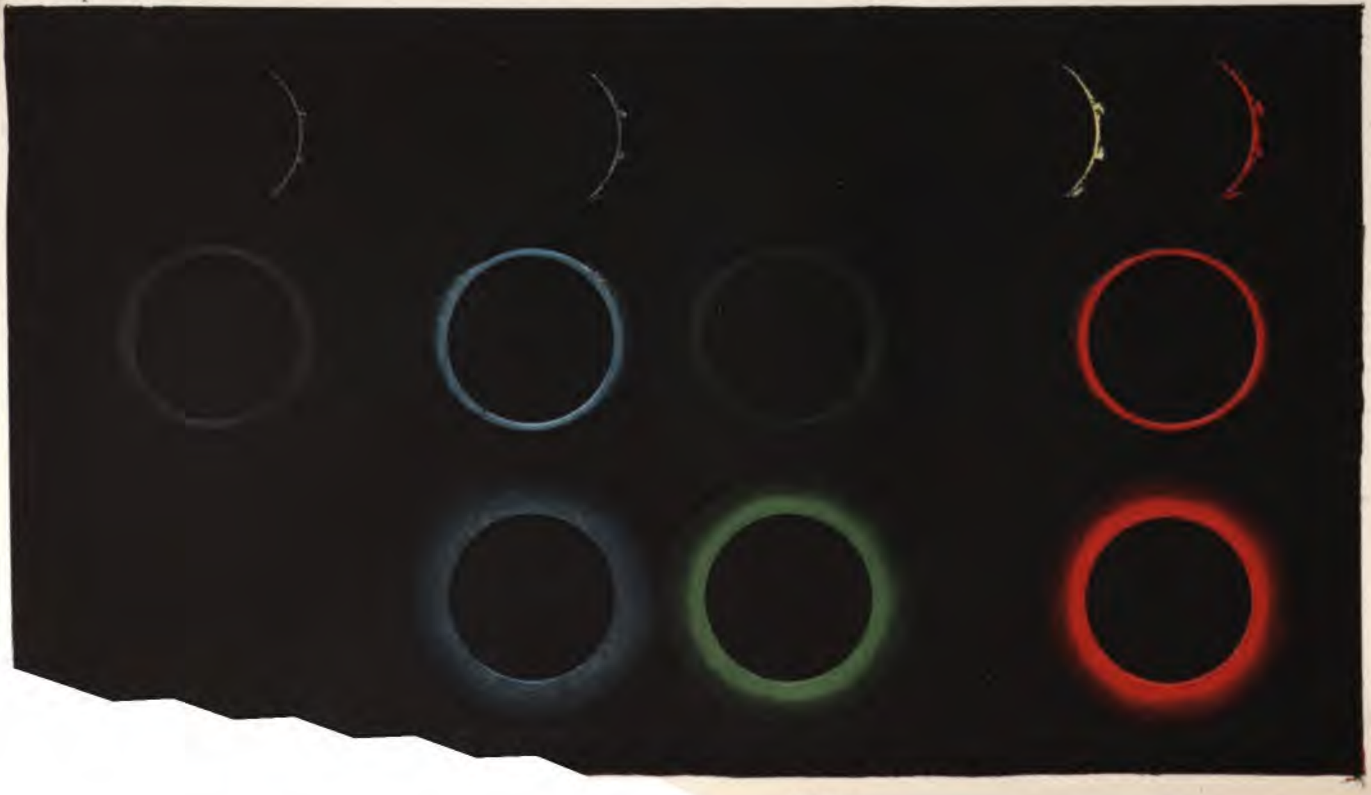
R. E. Allardice

Præmium inter discipulos II

Adjudicavit

G. Chrystal Professor.

Frontispiece.



CONTRIBUTIONS

TO

SOLAR PHYSICS.

I.

A POPULAR ACCOUNT OF INQUIRIES INTO THE PHYSICAL CONSTITUTION OF THE SUN,
WITH SPECIAL REFERENCE TO RECENT SPECTROSCOPIC RESEARCHES.

II.

COMMUNICATIONS TO THE ROYAL SOCIETY OF LONDON,
AND THE FRENCH ACADEMY OF SCIENCES,
WITH NOTES.

BY

J. NORMAN LOCKYER, F.R.S.

London:
MACMILLAN AND CO.
1874.

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THE CORONA OF THE ECLIPSED SUN 1871

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LONDON:
R. CLAY, SONS, AND TAYLOR, PRINTERS,
BREAD STREET HILL.

This Book is Dedicated to BALFOUR STEWART *and* P. C. JULES JANSSEN. *Encouraged by One Friend I undertook the Work which has brought me The Other.*

PREFACE.

THE present volume has grown out of an intention I formed about a year ago, to publish the Papers I had communicated to the Royal Society, dealing with the new facts which a new method of inquiry had revealed to us. I formed this resolution because, doubtless owing to their being scattered among the publications not easily procurable of a learned body, these Papers were evidently unread by some who were actually engaged, as well as by many who were merely interested, in the inquiry.

It next struck me that it would be wise to relieve the great—I fear too-great—terseness of these Papers by introducing into the same volume three Lectures I had also published in the Proceedings of the Royal Institution, giving an account of the first outcome of my inquiries, and of the results of the two Eclipse Expeditions which I had accompanied to the

Mediterranean and India. After I had determined to appeal by the publication of these to a wider public, I chanced to fall upon the notes of a course of four other Lectures on the Sun which I had also given in the Royal Institution, but which remained unpublished, and were almost forgotten. This decided me to attempt to make the story of the work more complete, and to add to it the information necessary for the general reader both as to the telescopic and spectroscopic sides of the inquiry, by means of these notes and some of the Essays which I had published at different times during the last ten years in *The Reader*, *Macmillan's Magazine* (these were written in conjunction with my friend Dr. Balfour Stewart), *Nature*, the *Times*, *Daily News*, and other periodicals.

This, then, is the origin of the work in its present shape.

In the First Part I have attempted to give a sketch of the various inquiries into the Physical Constitution of the Sun, and I have not hesitated to alter the arrangement of the four Lectures, and, in some cases, the Essays to which I have referred, so as to make the story a continuous one. I have not only largely added to the parts dealing with Spectrum Analysis, but I have given a body of information on this new science in its special relation to its solar applications which I hope may be found of value.

The Second Part, which consists of my Papers communicated to the Royal Society and to the French Academy of Sciences, of course is given *verbatim*, with the exception of the references to the illustrations, many of which have been transferred to the First Part of the book. To these Papers I have added Notes, on some of the more fundamental outcomes of the research, in which I have attempted to show the relations of my observations to those made by others; on the objections urged against some of the conclusions to which I had been led; and on the new facts acquired to Science by the labours of my fellow-workers. On this latter point, however, I have been compelled not only to give slight references where I could have wished to have given full details, but to cancel much matter actually written in order to keep the volume within convenient limits.

I must, however, thank Professors Young and Respighi for permitting me to largely increase the value of my book by referring at the length I have done to their observations. The faithful translation of the memoirs of the illustrious Italian observer here given, has been obligingly placed at my disposal by Mrs. G. M. Patmore.

Whilst the latter portions of this work have been passing through the press, a very important discussion has been carried on in the French Academy of Sciences, in which nearly every question raised by the new method of research in solar physics has been debated. I much regret that it has been impossible to include a notice of it in the present volume. It may, however, be remarked as satisfactory to English Science, that M. Faye, abandoning the theory of spots of which an account will be found in Chapter IV., has virtually adopted, in the essential point, that proposed by the English Observers.

My best thanks are due to the Proprietors of the Journals I have named for the readiness with which permission has been granted me to reprint; while in the matter of illustrations I have to express my obligations to the Council of the Royal Society; to Dr. Schellen, of Cologne, for the use of several woodcuts illustrating my own observations, the original drawings of which I sent over to him in 1869; to Mr. Westermann, of Leipsig, who added others to them; and to Professor Roscoe, who has allowed me the use of several which appear in his "Spectrum Analysis."

I am much indebted to Mr. Cooper for the care he has taken in the preparation of the plates and new woodcuts.

In the compilation of the Notes, and in revision of proof-sheets, my assistant Mr. R. J. Friswell has given me most valuable aid.

Up to the present time the spectroscopic examination of the sun has been regarded as the work of the astronomer and physicist, rather than of the chemist; and in England, though happily not abroad, many professional astronomers and physicists regard it, as a rule, as a matter of tenth-rate importance. I am sanguine enough to hope that, as time goes on, breadth of mind will take the place of the present more than apathy, and that chemists also will more generally interest themselves in, and aid, an inquiry from which, if I am not mistaken, they will learn much.

I cannot conclude this Preface without stating that had it not been for the aid afforded me by that admirable, but too little known, institution, the Government Grant Fund, and by my friend Dr. Frankland, who joined me in a branch of the research and generously placed his laboratory at my disposal, my observations would probably never have been made. Further, I admit and lament the incompleteness of them and of the book to which I have now consigned them. I know that the work I have attempted to forward requires a man who can give himself entirely up to it, while, less fortunate than many lovers of Science, the only time I have had to devote to these inquiries has consisted of fragments snatched from the leisure left me by my official duties. I have, however, the satisfaction of knowing that the method of observation which I have had a share in originating is rapidly taking root in other lands, and that it is being recognized as national work which the Janssens, Youngs, Respighis, Zöllners and Secchis of the future will carry on without break, for the instruction and benefit of mankind.

J. NORMAN LOCKYER.

Sept. 6th, 1873.

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137 line 3 from bottom, *for* incidently, *read* incidentally.
155 ,, 7 ,, *for* 1812, *read* 1802.
265 ,, 2 ,, *for* its, *read* our.
299 omit upper horizontal line from diagram.
376 line 2 from bottom, *for* radial of, *read* of radial.
411 ,, 12 ,, *for* phenomena, *read* phenomenon.
454 ,, 6 from top, *for* solar, *read* polar.

PART I.

A POPULAR ACCOUNT OF ANCIENT AND MODERN INQUIRIES INTO THE PHYSICAL CONSTITUTION OF THE SUN, WITH SPECIAL REFERENCE TO RECENT SPECTROSCOPIC RESEARCHES.

SOLAR PHYSICS.

WHAT IS A SUN?

CHAP. I.

AGES ago, when time was younger than it is now, and human curiosity had a whole virgin universe to revel in, when the stars were unmapped and space had been pierced by no sounding-line, what wonder that man, all ignorant of better things, looked upon the Sun—and worshipped it?

Although we know better than this now; although instinct has given place to reason, superstition to science, and curiosity to inductive philosophy, the mystery of the sun still remains, and we present earth-dwellers wonder at its mighty power with even greater reason than the men of old.

For, in fact, modern science, dealing with the whole field of the natural, and leaving the *super*natural out of the question, as science must ever do, has almost justified, so to speak, the instinct of those who added adoration to wonderment. The power they felt, we have proved to exist; the world-supporting, life-sustaining influence with which they invested the glorious orb of day, we now know

Early ideas.

belongs to it by right; man has measured its distance, has weighed it in the balance, and has glimpsed the forces at work on its surface; and he everywhere finds a stupendousness which baffles him and far surpasses anything the imagination of the early inhabitants of this planet could have conceived; and now that we know so much, rises as of old, towering above the mighty inquiries which have resulted in our present knowledge, the all-embracing question, "What is a Sun?"

Passing over the era of Sun-worship, which has left its mark in every land in which early man dwelt, let us endeavour to learn something as to the notions concerning the sun which were rife when the Western civilization began to dawn. We find as answers to the question "What is a Sun?" what we must designate as the wildest guesses, although some of them came from some of the greatest minds of antiquity.

Thales lived from 636 to 546 B.C.

Thales, one of the first of the Greek astronomers, whose early life was spent in Egypt, where he deeply studied the lore of the priests, had ideas concerning the sun which, it will be seen, were in the main sound; and it is curious that with such a good beginning subsequent philosophers went so far astray. He held that the sun and stars were of an earthy or solid substance, and that their light was due to fires fed by aqueous exhalations. He also explained correctly both solar and lunar eclipses, thereby implying that he thought that the moon shone by reflected light.

The anecdote of Thales falling into a well,[1] which is related by Plato, with the consequent saying of the Thracian female slave, that in trying to discover things in heaven he overlooked those beneath his feet, is widely known, and proves his popular reputation as an astronomer and star-gazer.

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[1] Plut. Plac. Phil. i. 3, as quoted by Sir G. C. Lewis in his admirable "Historical Survey of the Astronomy of the Ancients," p. 82, to which I am indebted for much information recorded in this chapter.

Anaximander, born 610, *died* 547 B.C.

But when we come lower down the stream of time, to 580 B.C., and seek what was then the prevailing opinion, we find Anaximander at the head of a school which explained the various solar phenomena by supposing that the sun and stars were bodies of condensed air, containing fire which escaped through certain apertures. He further imaged the sun as a wheel, the rays, forming the spokes, being emitted from the aforesaid apertures. His more detailed teachings in solar and lunar physics are thus stated by Sir G. C. Lewis:[1] he held that the sun was "of circular form, with an opaque annular band on its exterior, the circumference of which is twenty-eight times that of the earth; that within this annular band is a fiery central portion, equal in size to the earth; that the movement of the sun is due to its opaque ring, and that an eclipse of the sun takes place when the central aperture is closed. His hypothesis regarding the moon was similar: he supposed the luminous centre to be seen through a tube, like the mouth of a bellows; that the eclipses (or phases) are caused by the revolutions of the opaque ring. . . . . Hence Anaximander [unlike Thales] held that the moon shone by her own light."

Zenophanes of Colophon flourished 540 *to* 500 B.C.

Anaximander's notions, it will be seen, are not overburdened with clearness; and, indeed, about this time we get perhaps the greatest number of wild guesses. Zenophanes of Colophon taught that the sun was lit and extinguished every day, like coals, accounting for eclipses by the latter process; he told, moreover, of one eclipse which had lasted a month.[2]

Heraclitus of Ephesus flourished about 504 B.C.

The fault, however, found with Anaximander, cannot be laid at the door of the next suggestion to be recorded, which we owe to Heraclitus of Ephesus. This philosopher made himself noteworthy by the following ingenious ideas. He held that the sun and moon are bowl-shaped hemispherical cavities, with a bright side and a dark one. If the whole of the bright side of this hemispherical vessel is turned towards us, we see the uneclipsed sun and full moon. If the whole of the dark part is turned to us, then we see the sun eclipsed, and the moon is then new; and he further showed how, on this simple hypothesis, by imagining the bowls to be turned now a little one way and now a little the other, it was perfectly easy to account for the phases of the moon and the various kinds of eclipses of the sun.[1]

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[1] Op. cit. p. 93.

[2] Op. cit. p. 98.

The sun, according to this philosopher, was a body of compressed fire fed by exhalations; it was no bigger than a man's foot, and brighter than the moon in consequence of its position in a clearer atmosphere than that near the earth.

Anaxagoras of Clazomenæ, born 499, *died about* 430 B.C.

We must next refer to Anaxagoras of Clazomenæ, who was second to none among the Greek philosophers in his intense interest in cosmical speculations. He is stated to have said that he was born for the contemplation of the sun, the moon, and the heavens;[2] in fact his ideas on physical subjects were so advanced that he was accused of impiety. It is not a little curious that the charge of atheism which was preferred against him should not have been made before, for surely the charge of substituting mechanical force for the direct agency of the Gods would have applied equally to Anaximander or Heraclitus of Ephesus. And it is also curious that his doctrine that the sun was a mass of ignited stone larger than the Peloponnesus, instead of a God who drove his chariot across the sky, was one of the main charges against him. It is consoling to think that Pericles saved his life at all events by his eloquent defence.

Anaximenes, born 475 B.C.

Next, Anaximenes held that the sun, instead of being a globe, was flat like a leaf; "that the stars, being impelled by the resistance of the condensed air, cause the solstitial movements of the sun; that the nature of the moon is igneous, and therefore that she shines with her own light; that the earth is a flat trapezium; and that on account of this form it is supported by the air without sinking. He applied the same doctrine to explain the suspension of the sun and moon in space."[1]

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[1] Op. cit. p. 96.

[2] Op. cit. p. 103.

Empedocles flourished from 455 to 444 B.C.

Empedocles, the contemporary of Herodotus, held that there were two suns, one of fire in the lower hemisphere, and another, the upper one, "reflecting its own light from the fiery air upon the earth," as stated by Sir G. C. Lewis.

It is but fair to remark that all these ideas were held to be so wild by some of the best men among the Greeks that they rebelled very much against them; so that we find the whole genus of astronomical men soundly castigated by Socrates, who thought astronomy was desirable for determining the day of the month or hour of the night, but that to carry it further was waste of valuable time, and that "speculators on the universe and on the laws of the heavenly bodies were no better than madmen."[2]

Plato, 406 *to* 350 B.C.

It is not till the time of Eudoxus of Cnidos, the contemporary of Plato, that we find any sustained attempt at real investigation; and so impressed was he of the extreme importance of the sun to the earth, that he is recorded by Plutarch[3] to have declared that he would willingly suffer the fate of Phaethon could he only approach the sun near enough to determine its real figure and constitution.

Aristotle, 384 *to* 322 B.C.

By the time of Aristotle the right of free thought in cosmical matters had been thoroughly established, and we find that philosopher defining astronomy as a science founded on observation and calculation, although the idea that the sun and stars are living bodies eminently partaking of the divine nature occurs in his writings.[4]

From this time, throughout what we may term the pre-telescopic age, it would appear that the question of the physical constitution of the sun and stars gradually gave way to those which dealt with the various motions of each member of the starry host:

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[1] Op. cit. p. 96.

[2] Op. cit. p. 113

[3] Quoted in "Astronomy of the Ancients," p. 148.

[4] Op. cit. p. 163.

> "Cycle on epicycle, orb on orb,"

formed the key-note of a large part of astronomy after this time until we come, nearly two thousand years later, to Galileo and his contemporaries, when we again find ourselves in the midst of a sea of speculation on solar matters.

Needless to say that this was no accidental circumstance. The telescope had been discovered, and with it at the first essay the visible universe had been infinitely expanded. The period of the invention of the telescope is one of the most interesting and momentous in the scientific history of the world: in it the golden age of astronomy may be said to have dawned.

Kepler, Tycho Brahe, and Galileo.

We find then living on this planet three immortal men. First Kepler, who by his theory of elliptic motion at one blow swept away the elaborate work of twenty centuries on cycles, epicycles, and excentric motions.[1] Secondly, Tycho Brahe, whose admirable observations formed the groundwork of Kepler's investigations. Lastly, Galileo, who was at home in all branches of science; a man of tremendous mind; who independently, and as it were by the way, invented the telescope, and who was not only the first man who applied it to celestial objects, but perhaps the most illustrious man who ever did so.

We all know his reward. Like Anaxagoras, he offended against current dogmas, and in Christendom or Heathendom the penalty for that always was, nay almost *is*, the same.

According to Galileo's own story—and who shall doubt it?—he began the telescope's work on the sun in the month of October 1610,[1] being followed by the Jesuit Scheiner in April 1611, according to his own statement, and by Fabricius in June of the same year.

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[1] The words with which he sent his discovery forth are too admirable not to be quoted in this place:- "I have finished my book. It will be read by the present age or a future one—I care little which. It can well wait for a reader, for has not God waited 6,000 years for a contemplator of His works?"

Hariot observes spots in 1611.

It is pleasant to think that some of the earliest observations of the sun were made by a countryman of our own, and in the Tower of London. I allude to the eminent mathematician Hariot, who observed the spots as early as December 1611, though not as early as December 1610, as was once thought by De Zach.[2]

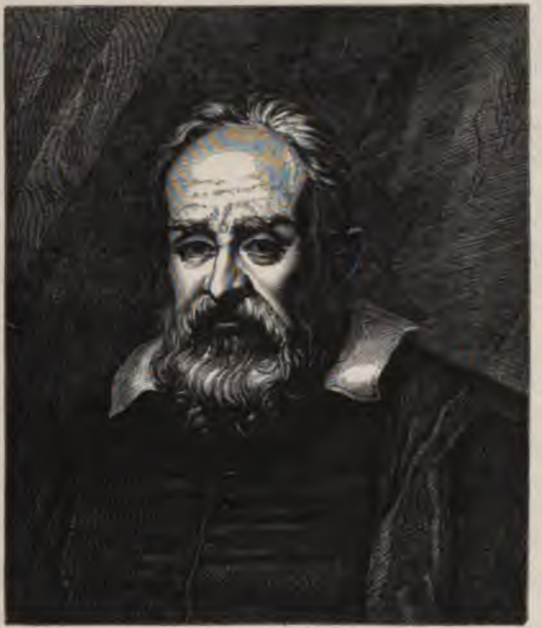

FIG. 1.—Galileo.

Needless again to say that the old-world answer to our question "What is a Sun?" was at once changed. It was no longer immaculate; no longer, as in the thought of Aristotle, a material image of the spotless purity of the divine mind. Galileo discovered spots on the brightly shining orb, and the thrill that ran through the world of Schoolmen as a result of this announcement may well be imagined!

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[1] See on the whole question of disputed priority in this matter, "Galilée, sa Vie, ses Découvertes, et ses Travaux." Par le Dr. Max Parchappe. Hachette, 1866. P. 92 *et seq.*

[2] Grant's "History of Physical Astronomy," p. 215.

Galileo's discoveries, thanks to the controversy which was raised by the Jesuit Scheiner, who wished to obtain all the credit for Galileo's work, have come down to us in full detail: for to defend himself, Galileo, in a remarkable series of letters to Welser, the chief magistrate of Augsburg, gives us the whole history of his work, which here, however, can only be lightly touched upon.

Galileo's first letter.

In his first letter, under date May 4, 1612, he states that the spots are real, as bright as the moon, and composed of matter not very dense and differently shaded; that they are not permanent, but resemble our clouds; and that it is absurd to think they are planets, which was Scheiner's opinion about this time.

Second letter.

In his second letter, dated August 14 in the same year, he refers to their common movements, and to their limitation to zones; states that they lie near the sun, and move on it and not above it; and then announces, from observations of them made near the sun's edge, first, that they are deep and of various depths, and secondly, that their blackness diminishes near the edge.

Third letter.

In his third letter, written on the 10th of December, 1612, he demonstrates the sun's rotation, and assigns a period not far from that given by modern observation.

Scheiner, who wrote under the signature of *Apelles latens post tabulam*, to the same Welser, held on his side that the spots were similar to Jupiter's moons, or probably to the strange things which Galileo had then recently discovered round Saturn, or that they might be comets. He held that it was impossible they could be on the sun itself, and imagined some to be as far from the sun as the Moon, Venus, or Mercury (on the Ptolemaic system). At the same time he pointed out that they are *thin*, to account for their oval appearance near the limb, adding that they are not fixed stars, although they are as dense as the moon.

It is not necessary in this brief sketch to do more than refer to the detailed work of Scheiner recorded in his "Rosa Ursina," or to the subsequent observations of Fabricius and Hevelius.[1] It is clear that the answer to our question was now much closer; indeed we have the spots and no longer the sun itself, due to evaporations and exhalations, the luminary itself being described as a "*liquor igneus . . . quasi vastissimum luminum pelagus et mare igneus, quod suos habet abyssos, occultos meatus voragines atque vortices.*"

Our next step carries us to 1774; for in De la Hire's memoir presented to the Paris Academy in 1704, there is nothing that calls for remark.

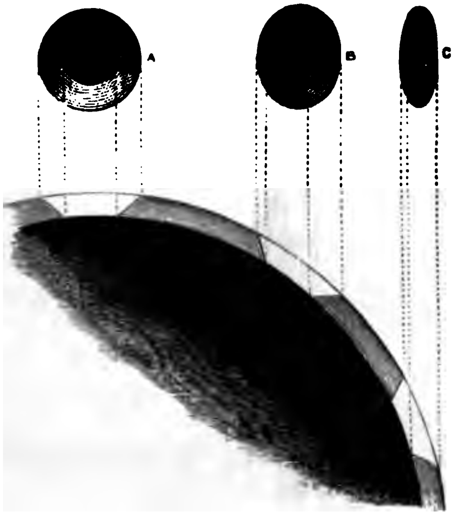

FIG. 2.—Explanation of Sun-spots on Wilson's hypothesis—of a solid, dark sun and cloudy envelope. A and C, spots as seen near sun's centre and edge. (*See text for explanation.*)

Wilson's work.

In 1774, Dr. Wilson of Glasgow communicated a paper to the Royal Society,[2] demonstrating that the spots were

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[1] The reader may with profit consult Grant's "History of Physical Astronomy," p. 213 *et seq.*

[2] Phil. Trans. 1744.

Wilson's work.

cavities in a luminous envelope surrounding the sun, which, according to him, was a dark globe. His observations were made on the great sun-spot of 1769. The reasoning on which he based his idea of the cavernous nature of the spots will easily be gathered from an inspection of the woodcut, Fig. 2: it will be seen that it depends upon the different appearances put on by the same spot as seen in the centre of the disc and near the sun's limb. In the case of a spot, supposed round, seen in the first position, it is clear that we shall have a round, black shade in the centre, equally surrounded by a half-tone; while when the spot is near the limb it is equally clear that on this hypothesis the central black shade will be almost entirely hidden, and the half-tone quite hidden on one side and largely developed on the other. Now this is exactly what is observed.

De la Lande.

To this paper of Wilson's, which is now acknowledged to be one of the most important contributions to our knowledge of the sun, in 1776 De la Lande replied in a paper presented to the Paris Academy. He does not agree with Wilson, although he gives up the idea which he had formerly held, that the spots were masses of scoriæ, and then he adds:—"J'ai donc pensé que les taches étaient plutôt les éminences d'un noyau solide, decouvertes et recouvertes alternativement par le flux et le reflux de la matière ignée où elles sont presque toujours plongées . . . les nebulosités [faculæ] qui environnent les taches et qui ressemblent à des bancs de sable présentent l'idée d'un bas-fond qu'on aperçoit à l'endroit où la matière fluide a moins de profondeur."

Sir Wm. Herschel's work.

We must last of all in this chapter deal with Sir William Herschel's[1] answer to the question at the head of it.

Aided by telescopes of his own manufacture, he doubtless was the first man on our planet to see the sun in all its beauty and detail, as it is now seen with even small instruments of modern make.

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[1] Phil. Trans. 1795.

The sun habitable.

Herschel's fixed idea, evidently one gained from his first observations, was that the darkness of a spot was an indication of a *cool habitable globe* under the shining envelope of the sun! and long before this theory is distinctly enunciated, some of the phenomena of the penumbræ are explained by the existence of "mountainous countries," though he acknowledges that such mountains must be at least 600 miles high.

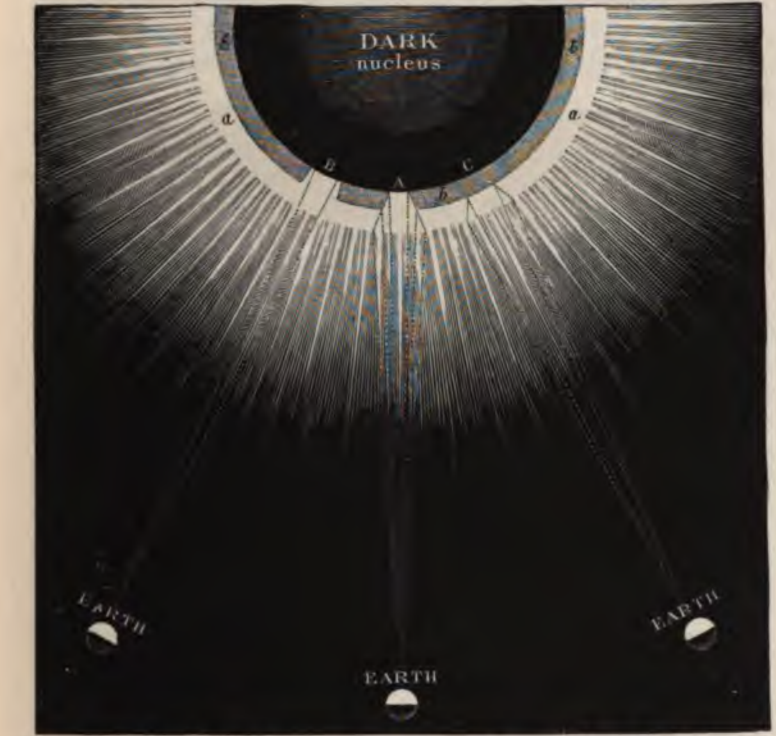

FIG. 3.—Herschel's theory of Sun-spots. *a a*, Photosphere, empyreal clouds; *b b*, Cloudy stratum, or planetary cloud envelope; A, Spot with nucleus (umbra) and penumbra; B, Spot with nucleus (umbra) without penumbra; C, Penumbra without nucleus (umbra), or "shallow."

Although it is possible that we may here trace the influence of the opinion of De la Lande, it is clear that he is a firm believer in Wilson's hypothesis, for the old nomenclature, which will be fully stated in the next chapter, is altered in Wilson's sense. Instead of "spots" we have "openings," while spots without nuclei are termed "shallows," connected faculæ "ridges," and separate faculæ "nodules:" the delicate mottlings of the general surface are also called "corrugations."

Herschel's hypothesis.

As a result of his labours, Herschel modified Wilson's hypothesis in this wise. Round Wilson's dark globe, which he considered inhabited, instead of one envelope he placed two, the exterior one consisting of empyreal, luminous or phosphoric clouds residing in the solar atmosphere; the interior one cloudy and opaque like our own, and highly reflective on its upper surface, so that the light and heat of the upper envelope were tempered, so to speak, to the solar inhabitants. He held that the solar atmosphere reaches to a great height, and is of great density, and that there is a clear space between the lower or "planetary" cloud envelope and the body of the sun.

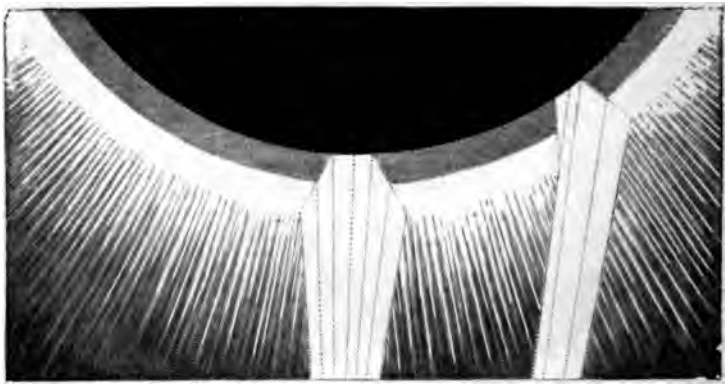

FIG. 4.—Herschel's theory of Sun-spots. Appearances presented by the same spot, as seen at the centre and near the limb.

This, then, was Herschel's idea: what the modern one is will be gathered from the following chapters.

ON THE TELESCOPIC APPEARANCE OF THE SUN.

CHAP. II.

IT is now more than two centuries and a half since the first telescope was turned upon the sun, and longer still since Fabricius, unmindful of Appian's recommendation of the use of coloured glass as a shield to the eyes, was wont to watch the wonderful spots crossing the solar disc. This he accomplished by allowing the sun's rays to enter through a small aperture into a dark room, and projecting its image

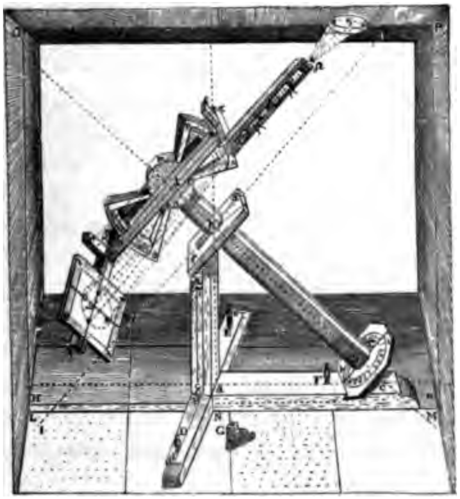

FIG. 5.—Early Sun-work. Scheiner's Heliotropium Teleoscopicum. Reduced copy of woodcut in his "*Rosa Ursina.*"

CHAP. II. on a piece of white paper, or, again, by viewing it directly when near the horizon. This latter method—the one pursued by Galileo in his observations—was improved upon by Scheiner, who placed a coloured glass between the object- and eye-glasses, and still more by Sarde, who in 1620 placed it between the eye-glass and the eye.

Here, then, we find the first employers of "optick tube" manfully battling against the obstacle which, even down to our own time, has proved all but insurmountable in observations of the sun—we mean his brightness, compared with which the lime-light pales its ineffectual fires, and appears even black when projected on his disc.

Methods of observation.

Among the attempts which have been made in later times to observe the sun satisfactorily, we are no longer limited to the employment of Sir John Herschel's glass speculum, or the diagonal eye-piece, both allowing nearly all the heat, and a very large percentage of light, to pass through them, and sending the enfeebled, but even still not sufficiently enfeebled, beam by reflection to the eye. But the light may be deadened by passage through a silver film on the object-glass, or by polarization more or less total in the eye-piece. It is perhaps to be regretted that the Dawes' solar eye-piece—the point of this latter instrument being, that the *quantity* of light admitted to the eye is reduced by observing only an excessively small portion of the solar disc—is falling into disuse. There is still another method of research which possesses many advantages, although it is a direct method, and as such may fuse or break the eye-piece, and give a distorted image; we allude to the use of a screen—introduced, as we have seen, by Fabricius—which has been employed so successfully by Messrs. Carrington, Howlett, and others, the screen being of any material—plaster of Paris is the best—and being placed at a varying distance from the eye-piece, according to the magnifying power required. Lastly to be recorded here—we shall have, further on, to return to its splendid results—is the art of solar photo-

Use of screen for solar observations.

graphy, which in the hands of Mr. De la Rue and Mr. Rutherfurd has shown us how inevitably it will some day supersede eye-observations of the sun.

More workers wanted.

Thanks to these modern methods of research, and the recent increase in the number of powerful telescopes brought to bear upon our luminary, we have already reaped a rich harvest of facts relating to the general surface, the spots, and the faculæ, which are gradually leading our philosophers to some more definite notion of the phenomena which these appearances indicate—although, alas! our telescopes are still all insufficient to translate to us all the agencies at work, and all the action going on in that wonderful globe some ninety millions of miles away. One thing: *we want more workers.* Many of our best observers are busy men, who more often see the sun through a glass darkly in their places of business, than by means of their instruments: and, again, *we want more powerful telescopes*, and we want these telescopes high above the lower strata of our atmosphere. Mr. Howlett's work was, we believe, all done with a 4-inch glass—would it had been an 8-inch! We must not, however, forget that much sun-work relates to the position of the spots, and not to their physical features. Kew and Ely formerly took spot-maps, so to speak, day by day by means of photography; but the sun-pictures obtained were small: at the present moment not only are we nowhere chemically registering spots to the noble scale of a yard to the sun's diameter, which Mr. De la Rue and Mr. Rutherfurd have shown us to be practicable, but to our disgrace the photographic record has stopped altogether.

The photographic record stopped.

Before we allude to the more recent discoveries in solar physics, it will be well to describe as briefly as possible the actual general appearance presented by the sun in a powerful telescope; always remembering that our mighty luminary is some 91,000,000 miles removed, that its diameter is 100 times that of our earth, and that the chasms we call sun-spots are sometimes large enough to swallow us up, and half-a-dozen of our sister planets besides; while if we employ the finest telescope, under the most favourable atmospheric conditions, we are only enabled observe the various phenomena as we should do with the naked eye at a distance of 180,000 miles.

The spots. The first things which strike us on the sun's surface, when we look at it with a powerful telescope, are the spots, which are not scattered all over the disc, but are generally limited to those parts of it a little above and below the sun's equator. The spots float, as it were, on the bright general surface of the sun, called the *photosphere.*

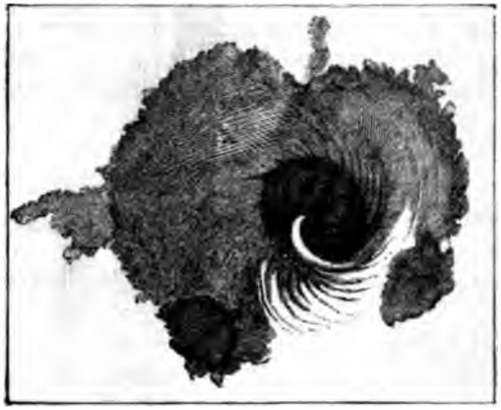

FIG. 6.—Cyclonic Sun-spot. (Secchi.)

Spots generally exhibit three shades of darkness, and float as it were in the bright surface or photosphere, the darkness increasing from the general surface till the apparent centre of the spot is reached. We have first the *penumbra,* then the *umbra,* then the *nucleus.* But sometimes the darker portions are excentric, and very irregular in outline.

Observations of the umbra and penumbra, with powerful instruments, have revealed to us the fact that *change* is going on incessantly in the region of the spots. Sometimes changes are noticed, after the lapse of an hour even :

here a portion of the penumbra is seen setting sail across the umbra; here a portion of the umbra is melting from sight; here, again, an evident change of position and direction in masses which retain their form. In some spots

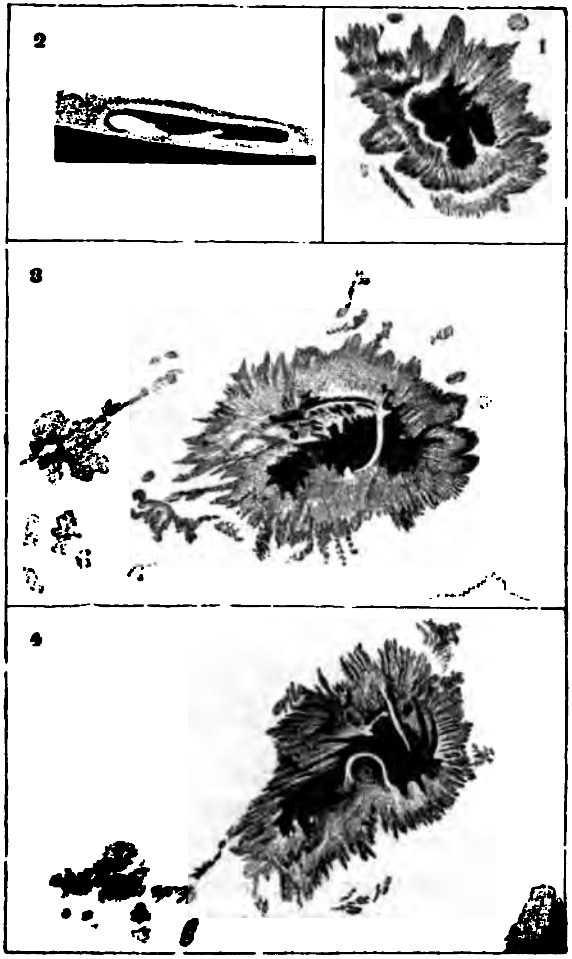

FIG. 7.—Changes in Sun-spots. The great Sun-spot of 1865 (Howlett). 1. The spot entering the Sun's disc, Oct. 7th (foreshortened view). 2. Oct. 10th. 3. Oct. 14th: central view, showing the formation of a bridge, and the nucleus. 4. Oct. 16th.

evidences of cyclonic action are very obvious (Fig. 6). The enormous changes, extending over tens of thousands of square miles of the sun's surface, which took place in the great sun-spot of 1865, are shown in the preceding engraving.

The faculæ.

We next come to the brighter portions of the general surface, which are well seen near the edge of the solar disc, and especially about spots approaching the edge; in these positions it is quite easy, even with a small telescope, to discern bright streaks of diversified form, quite distinct in outline, and either entirely separate or uniting in various ways into ridges and network. These appearances, which have been termed *faculæ*, are the most brilliant parts of the sun. Where, near the edge, the spots become invisible, undulated shining ridges still indicate their place—being more remarkable thereabout than elsewhere, though everywhere traceable in good observing weather. Faculæ may be of all magnitudes, from hardly visible, softly-gleaming, narrow tracts 1,000 miles long, to continuous complicated and heapy ridges 40,000 miles and more in length, and 1,000 to 4,000 miles broad. Ridges of this kind often surround a spot, and hence appear the more conspicuous; such ridges are shown in Fig. 8, but sometimes there appears a very broad white platform round the spot, and from this the white crumpled ridges pass in various directions.

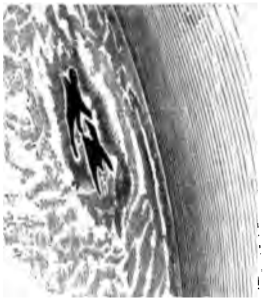

FIG. 8.—Faculæ surrounding a spot, as seen near the Sun's edge. (Noble.)

Magnitude of faculæ.

Mottling of the solar surface.

So much for the more salient phenomena of the sun's surface, which we can study with our telescopes. There is much more, however, to be inquired into. We may begin by saying, that the whole surface of the sun, except those portions occupied by the spots, is *coarsely mottled;* in a large instrument, it is seen that the surface is principally made up of luminous masses—described by Sir William Herschel as *corrugations*—and small points of unequal light, imperfectly separated from each other by rows of minute dark dots, called *pores*, the intervals between them being extremely small, and occupied by a substance decidedly less luminous than the general surface. The luminous masses present to different observers many varieties of irregular forms, and hence it was that a few years ago we had the famous "Willow-leaf Controversy," now at last put to rest by the more decided outcome of recent discoveries.

Mr. Nasmyth's "willow leaves."

The originator of this controversy was Mr. Nasmyth, who announced the discovery of willow-leaved things—which were afterwards suggested to be *solar organisms!*—nay, the definition ran still closer: they were *solar diatomaceæ*, covering, like so many scales, the whole surface of the sun. Aided by a refractor (of 8 inches' aperture, we believe) by Cooke, he found that—it is Sir John Herschel who narrates his discovery, and clothes it in his own beautiful language[1]—"The bright surface of the sun consists of separate, insulated, individual objects or things, all nearly or exactly of one certain definite size and shape, which is more like that of a willow-leaf as he describes them, than anything else. These leaves, or scales, are not arranged in any order (as those on a butterfly's wings are), but lie crossing one another in all directions, like what are called spills in the game of spillikins, except at the border of a spot, when they point for the most part inwards toward the middle of the spot, presenting much the sort of appearance that the small leaves of some water-plants or sea-weed do at the edge of a deep hole of clear water. The exceedingly definite shape of these objects, their exact similarity one to another, and the way in which they lie across and athwart each other (except where they form a sort of bridge across a spot, in which case they seem to affect a common direction, that, namely, of the bridge itself)—all these characters seem quite repugnant to the notion of their being of a vaporous, a cloudy, or a fluid nature. Nothing remains but to consider them as separate and independent sheets, flakes, or scales, having some sort of solidity. . . . . These wonderful objects have been seen by others as well as Mr. Nasmyth, so that there is no room to doubt of their reality. To be seen at all, however, even with the highest magnifying powers our telescopes will bear when applied to the sun, they can hardly be less than a thousand miles in length and two or three hundred in breadth."

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[1] *Good Words*, April 1863, p. 282.

When this discovery was announced, an observer of the highest eminence, the Rev. W. R. Dawes, at once denied the reality of these appearances; and a Paper by him, read at a meeting of the Astronomical Society, and the discussion upon it, gave rise to quite an excitement in the astronomical world. Mr. Dawes in his paper[1] remarked:—

The Rev. W. R. Dawes' observations.

"When carefully scrutinized with large apertures and high powers, under suitable atmospheric circumstances," solar phenomena "are so wonderfully different in their appearance from those presented by the diminished apertures formerly and necessarily in use, that it would not be very surprising if some observers, unaware of what had been previously seen and described, should imagine that the phenomena revealed by their newly acquired and powerful telescopes were really *new discoveries;* and this is what there is good reason to believe has in some instances actually occurred. Such a mistake may also be more

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[1] Monthly Notices, R. A. S., 1863, vol. xxiv. p. 33.

Dawes' observations.

likely to be made when a *new name* has been applied by some recent observer to an appearance long familiar to others, though previously unnamed. A name, no doubt, has the advantage of affording a convenient handle whereby to grasp the thing described; but, unless it is very correct and appropriate, it conveys an erroneous impression of the appearance to which it is applied, and may become far more injurious than advantageous. It seems to me, therefore, to be desirable to direct attention to what has been long ago observed and described; also to put on record some results of a pretty constant scrutiny of solar phenomena with powerful and excellent telescopes during the last twelve or fifteen years.

Mottled appearance of the sun's surface.

"The mottled appearance of the solar surface requires no very large amount of optical power to render it visible. I have often observed it with a good refractor of only 2½ inches' aperture and a power of 60. Examined with a large aperture, such as 6 or 8 inches, it becomes evident that the surface is principally made up of luminous masses imperfectly separated from each other by rows of minute dark dots—the intervals between these dots being extremely small and occupied by a substance decidedly less luminous than the general surface. This gives the impression of a division between the luminous masses, especially with a comparatively low power, which, however, when best seen with high power, is found to be never complete. The masses thus incompletely separated are of almost every variety of irregular form, the rarest of all, perhaps, being that which is conveyed to my mind by Mr. Nasmyth's appellation of '*willow-leaves*,' i.e. *long, narrow, and pointed.* Indeed, the only situation in which I have usually noticed them to assume anything like such a shape is in the immediate vicinity of considerable spots, on their *penumbræ*, frequently projecting beyond it irregularly for a small distance on to the *umbra*,—an appearance with respect to which, in my 'Description of a new Solar Eyepiece' which I read before this Society in April 1852 (see

'Memoirs,' vol. xxi. p. 161), I employed the following expression:—'The interior edge of the *penumbra* frequently appears extremely jagged; the bright ridges on its surface, which are directed nearly towards the centre of the spot, being seen projected to irregular distances on to the cloudy stratum (or *umbra*), and looking much like a piece of coarse thatching with straw, the edge of which has been left untrimmed.' After nearly twelve years of careful observation of the same phenomena, I do not think I could improve upon this description."

Mr. Dawes then refers to Sir John Herschel's description in his "Outlines of Astronomy," art. 307, where he says:—"The part of the sun's disc not occupied by spots is far from uniformly bright. Its ground is finely mottled with an appearance of minute dark dots or pores, which, when attentively watched, are found to be in a constant state of change. There is nothing which represents so faithfully this appearance as the slow subsidence of some flocculent chemical precipitates in a transparent fluid, when viewed perpendicularly from above."

Motion of portions of the photosphere near the spots.

Mr. Dawes endorses this description—with the exception of the constant change of the pores, which he denies, ascribing the appearance to tremors in the atmosphere—and goes on:—"A striking exception, however, to this comparative quietude is found in the immediate vicinity of spots which are either rapidly enlarging or closing. It is under these circumstances especially that the luminous masses are found to become more elongated. This is also more remarkably the case when they are *preparing for a rush across a chasm*, and thus forming those luminous bridges which so often intersect considerable spots. The point from which such a rush is about to be made is often indicated by a general crowding together towards that place, and a general inclination of the longer axis of each of the elongated masses in that direction, which might, as I imagine, be well exemplified by such chemical precipitates as Sir John Herschel alludes to, if they were about to flow through a narrow spout or opening in the vessel containing them."

Opinions of Dawes, Nasmyth, and Herschel.

The question was soon narrowed to this: Nasmyth declared for willow-leaves all over the sun; Dawes declared for something like them, which he calls "bits of straw" or "thatch," which are to be seen only round the spots where they are formed from the general flocculent covering of the sun, by a *drawing out* of it, as it were, by some action in their neighbourhood. Sir John Herschel still held to his old definition—"a slow precipitation of flocculent matter." It was not long before it was found out that the willow-leaves were "wanted." Mr. Pritchard told us, on the authority of Sir John Herschel, that "the advancing state of our philosophy required the willow-leaves: and they came just in the nick of time." Sir John Herschel remarks, in the article we have before alluded to: "These flakes, be they what they may, and whatever may be said about the dashing of meteoric stones into the sun's atmosphere, &c., are evidently the *immediate sources of the solar light and heat,* by whatever mechanism or whatever processes they may be allowed to develop and, as it were, elaborate these elements from the bosom of the non-luminous fluid in which they appear to float. Looked at in this point of view, we cannot refuse to regard them as *organisms* of some peculiar and amazing kind; and, though it would be too daring to speak of such organization as partaking of the nature of life, yet we do know that vital action *is* competent to develop both heat, light and electricity."

The flakes or willow-leaves the sources of heat and light.

M. Chacornac's observations in France.

The controversy which began in 1863 was still going on in 1865, with the distinct advantage that a rapidly increasing share of attention was given to solar phenomena. In France, M. Chacornac made important observations, while in England many good observers communicated papers to the Astronomical Society.

On the 2nd and 3rd of April there was a fine spot on the sun, which I was enabled to study under admirable atmospheric conditions, with the result that my own mind was soon made up on the controversy, and that important evidence as to a downrush into a spot was obtained.

My communication to the Astronomical Society ran as follows:—

"On the same leaf of the last number of the *Monthly Notices*[1] we have two opinions on the nature of the matter of which the sun's photosphere is composed, coming from two men whom we must all respect,—Father Secchi and Sir John Herschel. Father Secchi writes as follows:—

Secchi believes the photosphere cloudy.

"'I cannot divest myself of the opinion that the photosphere is really made up of clouds, and that the luminous stratum is actually constituted like our clouds, the only difference being that the clouds on the earth are of watery drops or crystals, and in the sun they are of some other substance.'[2]

Herschel believes the willow-leaves to be solid matter.

"Then we turn over the leaf, and we get Sir John Herschel's opinion:—

"' . . . . I suppose Mr. Nasmyth will have to give up the regularity of shape and equality of size of his "willow-leaves"—*clouds*, in the ordinary sense of the word, I do not think they are. . . . . I believe them to be *permanently solid matter*, having that sort of fibrous or filamentous structure which fits them when juxtaposed by drifting about and jostling one against another to collect in flocks, as *flue* does in a room.'[3]

"Now it is quite impossible that both these opinions can be right; and hence it is that observations such as those we have just listened to are most valuable at the present time. I am glad, therefore, to have the opportunity of bringing some very imperfect observations of my own before the notice of the Society, and I do so simply with a view of showing that the matter need not long remain in doubt. I must not, however, take any credit to myself as

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[1] Monthly Notices, R. A. S., 1865, vol. xxv. p. 236.
[2] Ibid., vol. xxv. p. 151. [3] Ibid., vol. xxv. p. 152.

M. Chacornac compares the photospheric matter to silver crystals.

being the first to observe what I am about to describe, because, as early as 1853, I believe, M. Chacornac made a similar observation. In a recently published letter[1] M. Chacornac remarks:—

"'Having had the occasion formerly to observe small pieces of silver bathed in borax, melting under the influence of the blowpipe, I have always in my descriptions compared the "crystals" of the photospheric matter to this silver solder in a state of fusion. Of the same opinion as Mr. Dawes, I hold that "straws" are the objects which

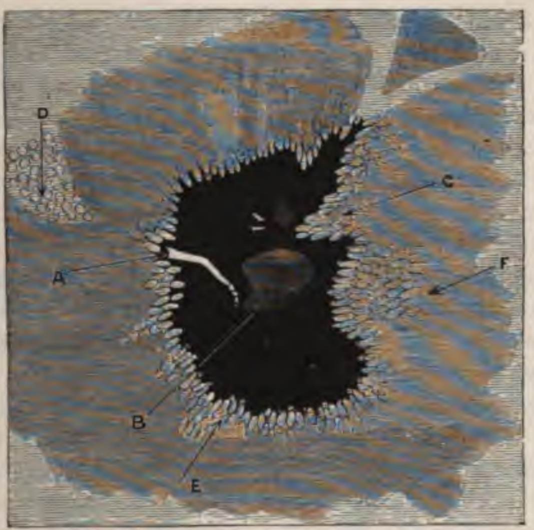

FIG. 9.—Sketch of Sun-spot completed about 11h. 40m. A.M., April 2, 1865.

The outlines of the penumbra and other appearances projected on to the umbra alone are intended to be shown.

A. Tongue of facula (?) stretching out into the umbra.

B. Clouds (?). See Figs. 10 and 11.

C. A promontory in which the "willow-leaves" are changing the direction of their larger axes with respect to the centre of the spot.

D. The "things" on the general surface of the sun (these are shown by the engraver too regularly and too near together), as opposed to the willow-leaves or rice-grains on the penumbra.

E. See Fig. 12.

F. Here the penumbra seems composed of layers, and the willow-leaves are arranged like feathers on a duck's wing.

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[1] *Reader*, Jan. 7, 1865.

Chacornac's observations.

give the best idea of the appearance of the objects of which the whole solar photosphere consists. On the other hand, I do not find that in the many notes, and especially in those of Father Secchi, containing observations of this nature, mention is made of any important phenomena presented by these incandescent "willow-leaves" or "rice-grains." As it is inherent in the nature of this matter, I will endeavour, as concisely as possible, to state in what it consists. If we observe a "crystal" of photospheric matter which is completely isolated and projected on a dark portion of a spot, for instance, it will be seen that for a certain time it diminishes in volume and becomes spotted over with small dark pores; that it is subdivided into numerous crystals, as if the photospheric matter were being volatilized, or as if there were a re-absorption going on, absolutely in the same way as crystals of sugar melt under a current of steam.'

"I also have been able to see this diminishing of volume; but I have not been able to see the dark pores. From Mr. Fletcher's paper I gather that he has seen them. The spot to which I wish to draw particular attention was observed on the 2nd and 3rd April. The observation of the 3rd was very cursory indeed; in fact, my attention was drawn entirely away from the spot by the general appearance of the sun. Of this more presently. On the 2nd of April the spot was extremely remarkable. It was a spot of the normal character, by no means cyclonic, but with a tongue of what appeared to be a portion of facula —I judge so from its extreme brightness—stretching half way into the spot, as it were. When the observation commenced, about half-past eleven, the tongue of facula was extremely brilliant; by the time I left the telescope, about one o'clock, that same tongue of facula seemed to be less brilliant than any portion of the penumbra. At the same time it seemed to me to be 'giving out,' as it were, at its end, and a portion of the umbra between it and the penumbra appeared to be veiled with a stratus

Changes in brilliancy of facula.

cloud evolved out of it. After a time large, very dim 'willow-leaves' seemed to be forming (condensing) on the following portion of the cloudy mass. So that at first you got a very brilliant mass of what appeared to be facula gradually melting away into umbra, and then the umbra condensing into 'willow-leaves.' I know that, in the case of an observation of this extremely delicate kind, it would be rash to say that one absolutely puts the right reading on what one saw; but I certainly feel convinced in my own mind that I saw a bright mass of facula—I say facula on account of its brightness, but I do not think it was actually above the level of the photosphere—gradually expand itself into a cloud, and then the cloud became edged with 'willow-leaves,' which increased in number. At one time I saw two, clearly and distinctly. Some little time afterwards I saw five or six. I imagined I had made a mistake; but I do not now think it possible, for the atmospheric conditions were extremely good; and I had well scrutinized the region before. I was also enabled to watch three or four cloud-masses on the inner edge of the penumbra detach themselves from it at different points, and traverse the umbra towards the centre of the spot. Now, in a cyclonic spot this is easily intelligible, but it is difficult to understand two currents opposed to each other, or at right angles to each other; the one carrying the cloud-masses across the spot,—say from right to left,—another carrying them up or down. This, however, is what I distinctly saw. I hope that observers who have better

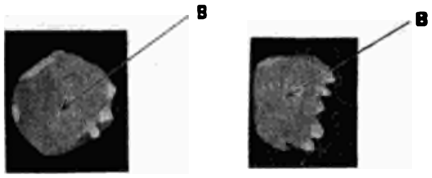

FIG. 10.—12h. 30m. FIG. 11.—12h. 55m.

The cloudy portion seems condensing at its edge into denser masses, but they are very dull and faint.

The facula seen to expand into a cloud.

eyes and better instruments than mine will bring their attention to this subject.

"It has been taken for granted at present by MM. Faye,[1] Chacornac, and others, that there is a downward current into a spot, and my observations, I think, show this down-rush. It will be of great importance if the facts—first, that there is a down-rush; second, that this is accompanied by the melting of the cloud masses carried down—can be established.

Change of direction of the willow-leaves.

"I also saw the 'willow-leaves,' or 'rice-grains,' in one region of the penumbra change the direction of their longer axes in about three-quarters of an hour with regard to the centre of the spot—in fact they turned round bodily through a considerable angle. Others projected on the umbra gradually melted away out of sight. One willow-leaf I distinctly observed to set sail, as it were, over the umbra, and it had travelled a considerable distance when I parted company with it. I believe I saw another condense—the small one in the right-hand corner of the drawing (Fig. 9).

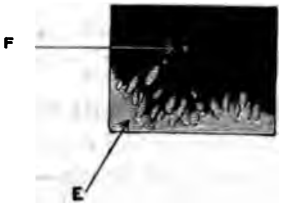

FIG. 12.—12h. 30m.
"Willow-leaves" detaching themselves from the penumbra.
A very faint one at F.

Willow-leaves sail across umbra.

"These were some points of interest observed on April 2. Early on the following day the definition was, if possible, very much more fine. 400 was a power easily used on the 2nd, and on the morning of the 3rd, the sun bore my highest power—690—an eye-piece which one uses but rarely to advantage, with my aperture (6¼ inches Cooke). With this the surface of the sun was distinctly seen, and, thanks to the London fog, which cut off the heat-rays and allowed the object-glass to do its utmost in the way of definition, I did not require any dark glass. Drawing No. 9, which was hastily sketched on the 2nd of April, shows the great difference in shape and appearance of the 'things' (whatever they may be) on the general surface of the sun, in contradistinction to the 'willow-leaf'[1]—or 'rice-grain'—things visible in the penumbra. This great difference was more than ever established in my mind by what I was enabled to see on the 3rd.

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[1] So runs my paper in the Monthly Notices, but I fear I have done M. Faye an injustice, as ever since, until January 1873, when I add this note, he has advocated an uprush in spots.

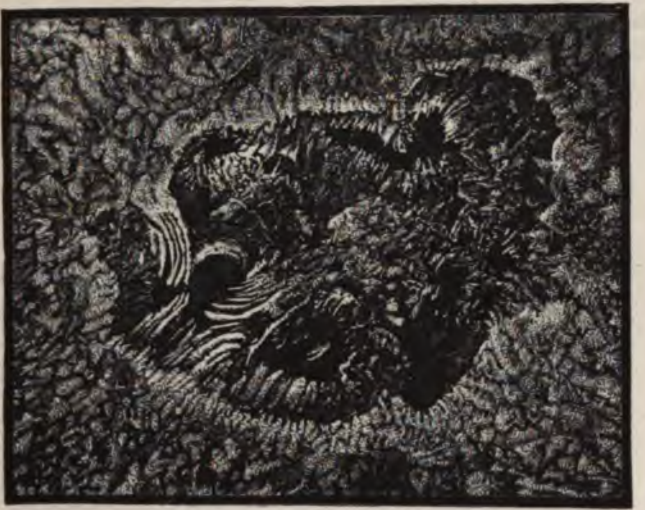

FIG. 13.—Sun-spot (Secchi), showing the "straws" in the penumbra, and the irregular masses on the general surface.

"I am well aware that my opinion on a matter like this is not of great value; but still it may be worth while to

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[1] These words are used merely with reference to the shapes they indicate; the second edition of Mr. Nasmyth's "Willow-Leaves" includes the stumpy rice-grain. The difference between the actual size of the rice-grain and Mr. Nasmyth's estimate of the size of the willow-leaves is not in question.—J. N. L. (Note added 1865.)

CHAP. II.

point out that I did see 'willow-leaves' or 'rice-grains' pretty regularly shaped in the penumbra, and therefore might be expected to have seen them on the general surface of the sun had they been there. But I certainly saw nothing of the kind on the general surface of the sun. It would seem indeed that there is a running down of the shape; for whereas on the sun the thing in debate is, according to my observation, a confused sort of circular mass, you get in the penumbra near the edge of the photosphere sometimes pointed, sometimes rounded, sometimes truncated cloud masses, with a sharpened portion towards the umbra, and a very blunt portion towards the general solar surface. But if you observe similar masses which have traversed the penumbra, you find generally that they are pointed at both ends, the point being sometimes rounded, sometimes truncated. Of course it would be extremely difficult—indeed it is an observation one can scarcely even hope to make—to watch a portion of photospheric matter coming in with a rush, as it sometimes does into the penumbra, then travelling through and changing its shape,

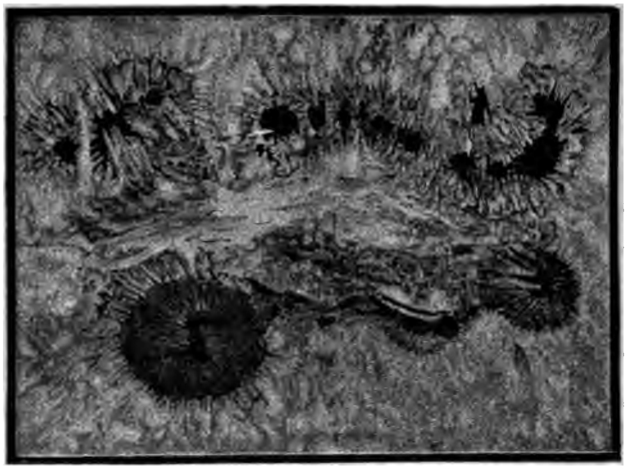

FIG. 14.—Sun spot. (Secchi.)

Different appearance of masses after they have crossed penumbra.

and then finally setting sail and melting; but I venture to think that, if that can be done, it will possibly be found that the confused circular mass which is seen on the general surface of the sun will be gradually drawn out in its journeying towards the umbra; and if you can get it to traverse over the umbra, you will find that it will look as much as possible like a willow-leaf."

Since this was communicated to the Astronomical Society Mr. Huggins has given his verdict very much to the same effect.

Why we see faculæ.

Now it has been satisfactorily proved, notably by a beautiful stereoscopic combination of them, suggested by Mr. De la Rue, that the faculæ are higher than the general surface of the sun; that is, where the clouds are highest they appear brightest—*we see faculæ*—because they extend high into the absorbing atmosphere; we know that on the bright surface of the sun rests an absorptive atmosphere, because the luminosity is remarkably less near the border, indicating that there is a greater thickness of atmosphere there which the light has to pass through before it reaches our eye. This point will be returned to in the sequel. The more minute features—the granules—are most probably the dome-like tops of smaller cloud masses, bright for the same reason that the faculæ are bright, but to a less degree; the fact also that the granules lengthen out as they approach the umbra of a spot is similar to the effect observed in the clouds in our own sky lengthened out when they are drawn into a current; while the admirable drawings by Secchi reproduced in the preceding pages show how plastic the photospheric matter was, to be thus torn and contorted.

But we shall see as we go on what a flood of light has been thrown on all these matters during the last few years.

MR. CARRINGTON'S RESEARCHES ON SOLAR SPOTS.

CHAP. III.

SPOTS on the Sun! We can little realize nowadays all the hardihood required to make that assertion in Galileo's and Scheiner's time. Spots on that body which Aristotle the great master had declared to be the type of everything immutable and incorruptible; maculæ on the immaculate! spots on the last stronghold of the spotless! What wonder that even down to our time all the horror set agoing by that daring statement has not yet left off vibrating.

It is now more than 200 years ago since Scheiner, one of the first employers of the astronomical telescope, published his great book on the sun, in which these matters were first laboriously investigated. Since his time, especially in later years, many observers, and among them Schwabe, Wolf, Peters, and Laugier, have continued the work; but the wonder and astonishment which they call forth are not yet one whit diminished to such men as Herschel, and Helmholtz, and Thomson; nor are they the least part of that seemingly invincible mystery which surrounds the glorious sun, whose mighty power at last seems dawning upon us *terricolæ*.

Newton's query.

Newton, in his times, was content to ask, "Are not the sun and fixed stars great earths vehemently hot?" and some 200 years later, in our own, Mr. Carrington is still driven to the question, with which we started this book, "What is a Sun?" Now this question is a generic one, embracing an infinitude of specific ones of more or less importance. Thus, for instance, we want to know something of its orbit-sustaining powers, and of the origin of these powers; looking at it, as a "great earth," we want to know when it will be as cool as ours is—as a star, if it be a variable one either in light or colour. Looking at it, again, as a sun, we want to know all its conditions, the secrets of its light and heat, of solar physics generally, and of the aforesaid spots, which, like straws on a stream, tell of the wondrous forces at work. And it is to learn something of these spots that Mr. Carrington has been content to observe the sun every fine day for some seven years and a half, and to deduce the exact position of the spots observed. This he has done with a very definite object in view, and one which necessitated a forsaking of apparently all the most interesting kind of work connected with their telescopic appearance. He writes:—"The distribution of radiative power, the position of the thermal equator, the numerical amount of illuminating power and its possible variations, the estimation even of the degree of energy exhibited in the production of spots, and many other features, were consciously left aside, and the subject before my mind reduced pretty much to tracing regularity in the distribution in the maculæ, detecting the true period of rotation of the body of the sun, and the determination of the systematic movements or currents of the surface, if such exist, in any definable manner."

Carrington's work

The volume in which these researches appear, though a big book, is not the biggest we possess on this subject; but De Lambre tells us that the biggest—Scheiner's "Rosa Ursina"—should have rather consisted of 50 than 784 pages: so Mr. Carrington may take comfort.

Mr. Carrington,[1] distancing all previous inquirers in the perfection of his instrumental means, the methods of reduction employed, and the time he has given to the subject, presents us with values of the sun's elements, and of the time of its rotation, which it will be very difficult for future astronomers to improve upon.

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[1] "Observations of the Spots on the Sun, from November 1853, to March 24, 1861, made at Redhill." By R. C. Carrington, F.R.S. Illustrated by 166 plates. Williams and Norgate.

Phenomena of earth's rotation.

In order that we may best communicate what he has done, let us suppose a boundless ocean in which both earth and sun are half immersed. This will represent to us the plane of the ecliptic, or earth-plane. Let us further suppose both earth and sun to be rotating on their axes in a certain period of time, the axes being either upright or tipped down—*i.e.* inclined—to a certain extent in a certain direction.

Now, in the case of the earth we know exactly all these particulars. We know that our day results from, and is an exact measure of, our rotation; that our seasons are caused by a certain tipping down of the axis, and that the pole-star marks for us, with sufficient accuracy for our present purpose, the exact amount and direction of this tipping down. The annexed woodcuts (Figs. 15—18) will give a good idea of these particulars in the case of our earth, and how its appearance changes in consequence as seen from the sun.

Equally, therefore, before we can properly define our sun as a member of our system, we must find out these particulars for it; we must know how it also is floating in our hypothetical ocean, whether at rest or perpendicularly, and if neither, then the length of its day, to speak in earthly language, and the position of its pole-star must be determined. But how is this to be accomplished? We need scarcely say that it is to the spots on the sun that we must look for the solution of these problems. Fortunately, the sun, when examined with a telescope, is not the equally illuminated disc it appears to be to the unaided vision, and the spots visible on its surface may be likened to straws which show us the rate and direction of a stream, for no sooner were they discovered than it was observed that they had a motion across the sun.

Now let us return to our hypothetical ocean, and see how

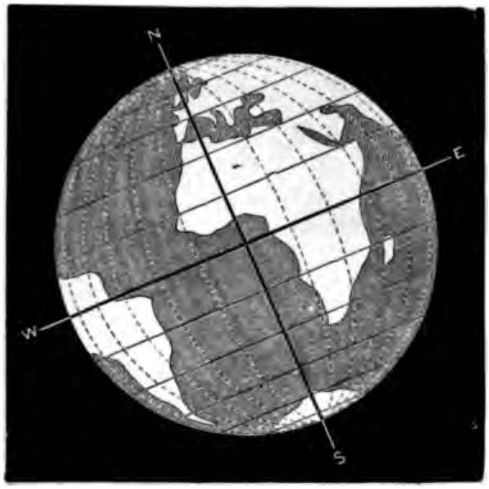

FIG. 15.—The Earth, seen from the Sun (Vernal Equinox, noon at London).

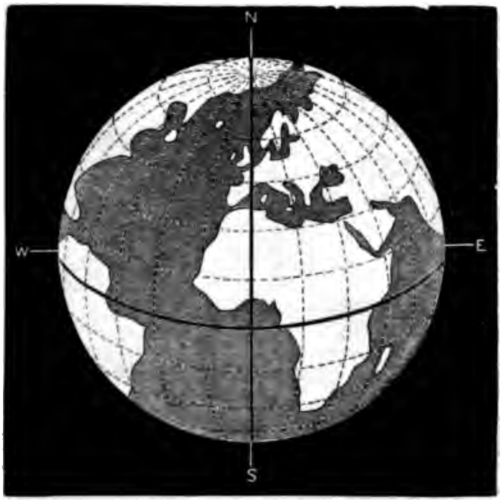

FIG. 16.—The Earth, seen from the Sun (Summer Solstice, noon at London

we can take advantage of this knowledge: if our half-immersed sun were floating quite uprightly, the spots

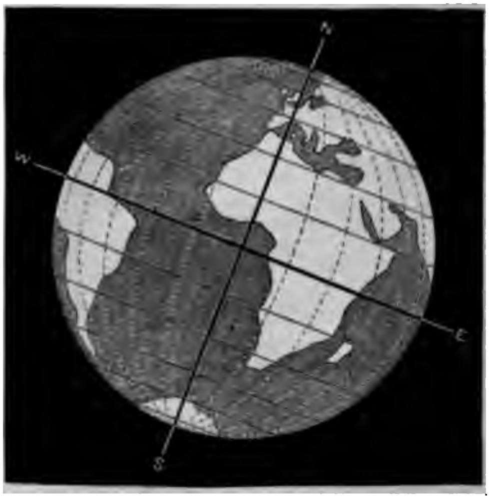

FIG. 17.—The Earth, seen from the Sun (Autumnal Equinox, noon at London).

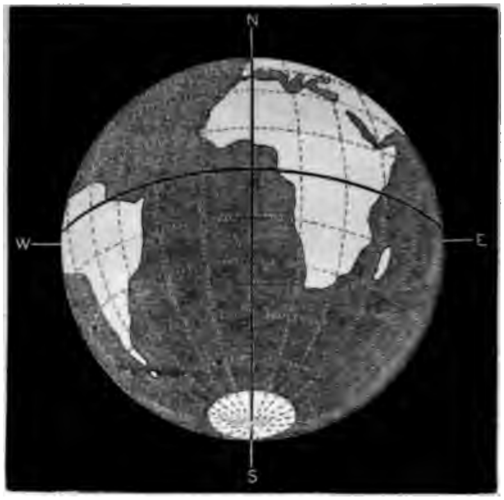

FIG. 18.—The Earth, seen from the Sun (Winter Solstice, noon at London).

carried round by its rotation would always keep at the

Effects of inclination of axis.

same height above the waves, from whatever part of the earth's annual journeying the sun was observed. But this we do not find to be the case. From two opposite points of the earth's orbit—the points it occupies in June and December—the spots are seen to describe straight lines across the disc; while, midway between these points, in September and March, these paths are observed to be sharply curved, in one case with the convex side upward, in the other with the convex side downward. A moment's thought will show that these appearances can arise only from a tipping down of the sun's axis, the amount and direction of which can be ascertained by observing either the angle made by the parallel paths which the spots describe with the surface of our ocean, or the amount of curvature of the curves, and by noting the earth's place when the lines are straightest or the curves the most *prononcées*. It is thus found that the sun's axis inclines towards the point occupied by the earth in September.

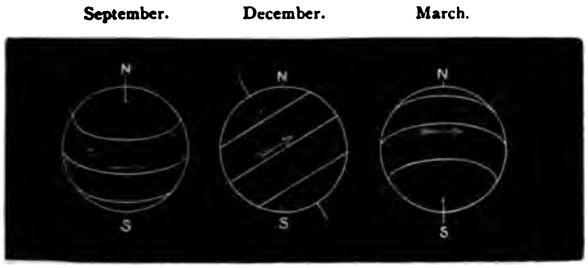

FIG. 19.—**The Sun seen from the Earth.** Position of the Sun's axis, and apparent paths of the spots across the disc, as seen from the Earth at different times of the year. The arrows show the direction in which the Sun rotates. The inclination of the Sun's axis is exaggerated, so that the effect produced may be more clearly seen.

Here, then, we find Mr. Carrington sitting down to work in order to record every spot visible, whether small or great, on the day of observation. To get a view of the sun he projected an image, as did Scheiner with that first of equatorials, his *Heliotropium teleoscopicum*, and as, doubtless, many of our readers have done, on a screen;

but how was the position of the spots to be accurately recorded? There was in 1855 no Kew photoheliograph to do this; but the question received at Mr. Carrington's hands a satisfactory solution almost as soon as suggested, and the eyepiece was armed with two cross wires, very nearly at right angles to each other, and inclined approximately 45° on each side of the parallel of declination. The why and the wherefore of this beautiful contrivance are mathematically demonstrated and fully explained by Mr. Carrington.

Carrington's arrangement for finding position of spots.

The telescope, when adjusted in declination so that the image of the intersection of the cross bars would fall nearly on the centre of the image of the sun, was clamped; and the image of the latter was allowed to travel over the fixed images of the bars, the exact times of contact, true to the tenth of a second, at which the limbs and spots touched both bars being noted. Sometimes as many as thirteen spots were thus observed at three passages over one bar and two over the other.

Nothing can surpass the wonderful patience with which Mr. Carrington—our English Schwabe—has thus collected thousands upon thousands of observations, or the consummate skill with which they have been reduced. It is, perhaps, from the 166 plates in which Mr. Carrington with the most scrupulous care has represented the spots visible on each day of observation, that his diligence will be most tangibly gathered. The exact position of each group on the sun in reference to its equator and the assumed prime meridian is given in one series; in another all the observations of each group observed more than once are fully shown, the altered appearances of the spots being given as well as their different positions on the disc.

And here we approach one of Mr. Carrington's results, apparently a very simple one, but one that would amply repay him for all his labour were it to stand alone.

All our text-books tell us that the sun turns on its axis, the period of its axial rotation having been deduced

Time of the sun's rotation on its axis.

from observations of its spots noticed indiscriminately in any part of the disc. But, from the time of Galileo, who made the period of rotation about a lunar month, down to our own, authorities have differed very considerably. Thus Grant, in his "History of Physical Astronomy," gives a period of 27d. 8h. (he quotes no authority). Laugier found 25·34d., and later observers have made it still less.

'Proper motion of spots.

Mr. Carrington now comes to the rescue, and announces to the world a discovery of the utmost value. He tells us ***the spots travel at different rates, depending upon their distance from the equator,*** either north or south, and that the different rates are bound together by a law, so that he is enabled to represent all the rates very nearly by the following formula:—

$$865 - 165' \sin.^{\frac{7}{4}} (l - 1^\circ)$$

So that the sidereal rotation of the equatorial photosphere is accomplished in 30·86 days; and of that at a latitude of 50° N. or S.—the highest point at which spots have been observed—in 28·36 days.

We said of the photosphere: the sun itself, whether it be the glade-bedeckt world imagined by Sir W. Herschel, or the incandescent globe required by both the old and the new philosophies, has revealed none of its secrets to Mr. Carrington. But it is clear that it must be content with one only of these differing rates of motion: and the question is, what is it? Sir John Herschel, in an admirable article on sun-spots in the *Quarterly Journal of Science,* deals with this question. Mr. Carrington considers that the views of Professor (now Sir William) Thomson "On the Mechanical Energies of the Solar System" are supported by his discovery, supposing that the sun itself travels more slowly than the equatorial photosphere. He remarks:—"In the absence of an impressed motion from some such external force, it would be expected that the currents of the surface of the sun would resemble those of the earth's ocean and atmosphere, and be westerly and towards the poles in the tropical latitudes, and easterly in the higher latitudes; the direction of rotation in such cases being the same, and the equatorial region in each the hottest." Besides determining anew the elements of the sun's equator—in other words, the position of the sun's pole-star—Mr. Carrington has put us in possession of an important fact regarding the minimum period of sun-spots.

Contraction of limiting parallels at minimum spot period.

He detected "a great contraction of the limiting parallels between which spots were found previously to the minimum; . . . . and soon after this epoch the apparent commencement of two fresh belts of spots in *high latitudes*, north and south, which have in the subsequent years shown a tendency to coalesce, and ultimately to contract, as before, to extinction." In Sir John Herschel's paper,[1] to which we have before alluded, there is a passage which shows in a very strong light the value of these remarks of Mr. Carrington's. In attempting to account for the phenomena of sun-spots by the presence of a nebulous ring,

Herschel's meteoric ring theory.

he writes:—"Let us suppose (and such a supposition has not been deemed inadmissible in attempting to account for the periodical return of meteors) the existence of an elliptic ring of vaporous, nebulous, or small planetary matter, which such a major semi-axis (4·979) as corresponds to a periodic time of each of its particles = 11·11 years; of such eccentricity as to bring its perihelion within the limits of the solar envelopes; and revolving either in the plane of the ecliptic or in some other plane at a more considerable inclination of the sun's equator. Let it be further assumed (still in anlaogy with assumptions not regarded as unreasonable in the meteoriferous ring), that the distribution of the circulating matter in it is not uniform—that it has a maximum and minimum of density at nearly, but not quite, opposite points, and no great regularity of gradation between them. It is very conceivable that the matter of such a ring, introducing itself with planetary velocity into the upper and rarer regions of the sun's atmosphere at an incidence oblique to its regular and uniform equatorial drift, might create such disturbances as, either acting directly on the photosphere, or intermediately through a series of vortices or irregular movements propagated through the general atmosphere, should break its continuity and give rise to spots, conforming in respect of their abundance and magnitude to the required law of periodic recurrence. If the change of density from the maximum to the minimum were gradual, but from the minimum to the maximum more abrupt, so as to allow the disturbances to subside gradually and recommence abruptly—the fresh and violent impulse would be delivered first of all on a region remote from the equator (by reason of the obliquity of the ring), and would give rise to a recommencement of the spots in comparatively high latitudes.

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[1] Quarterly Journal of Science, April 1864.

Section of ring at its aphelion nil

"If the section of such a ring as we have supposed at its aphelion were *nil*, the period of 11·11 years would be strictly carried out; the maxima and minima would succeed each other with perfect regularity, and the paucity and abundance of the spots in the several phases of the same period would follow a fixed ratio. But if not, the several parts of the ring would not revolve in precisely equal times —the period of 11·11 years would be that of some dominant medial line, or common axis of all the sections in which a considerable majority of its matter was contained; and the want of perfect coincidence of the other revolutions would more or less confuse without obliterating the law of periodicity, which, supposing the difference to be comprised within narrow limits, might still stand out very prominently. Now, it might happen that there were two such medial lines, or more copiously stocked ellipses, each having a maximum or minimum of density, and that their difference of periodic times should be such as to bring round a conjunction of their maxima in 56 or any other considerable number of years; and thus would arise a phenomenon the exact parallel of Dr. Wolf's long period and his series of greater and less maxima."

The value of a well-ascertained fact.

We have given this extract to show the enormous value of a *single well-ascertained fact;* and Mr. Carrington may be congratulated upon the possession of that sagacity which, by limiting his inquiry, has enabled him to produce such facts. But to show how wide is the field laid open to us by any facts connected with the sun, we may state that, in his last plate, Mr. Carrington gives two curves showing the variations of spot-frequency in the 11·2 years period discovered by Wolf, and the variations of the distance from the sun of the planet Jupiter,—and truly the curves run together in a very marvellous manner for a certain distance; but more recently still Dr. Balfour Stewart has attempted to account for the long sun-spot period of about fifty-six years, which in such a remarkable manner connects the increase of sun-spots and the frequency of auroræ. He remarks, that as two revolutions of Saturn are very nearly equal to five of Jupiter, we shall have every fifty-nine years the same planetary phenomenon repeated; and he shows that the dates of the aphelion of the two planets come closest together about 1840, which is not far distant from 1836, the date of maximum given by Professor Wolf. There will be more to say on these planetary connections presently.

Curves of sun-spots and of Jupiter's distance compared.

This work—this splendid addition to our astronomical literature and knowledge—is, we must state in conclusion, after all but a kind of *hors d'œuvre* undertaken to fill up those parts of the day which were not required for the reduction of the nights' observations made for the Redhill Circumpolar Stars Catalogue. Looked at from any point of view, it reflects the highest honour, not only upon its author, but upon the Royal Society, who have aided its publication by a grant, and upon the whole body of English amateur astronomers. It is well that all should know that there are such men as Mr. Carrington, who are content to give their time, money, energies, talents, everything they possess, to the pursuit of such studies, and, while Kew and Ely are daily registering the sun,[1] so as to extend the usefulness and gather fresh facts by means of the methods here laid down, we hope that some one will be encouraged by Mr. Carrington's bright example to study the physical features of the spots apart from all theories, and present us with the detailed telescopic appearances which they present at intervals of—say—some half-hour or so. Who will volunteer ?

[1] Alas ! this is no longer true ! to the lasting disgrace of British Science the daily photographic record has ceased. (1873.)

M. FAYE'S FIRST THEORY OF THE PHYSICAL CONSTITUTION OF THE SUN.

CHAP. IV

LITTLE wonder is it that the physical constitution of that luminary in which modern science teaches us our very life itself centres, should be at present a subject of all-absorbing interest among philosophers; and little wonder, too, is it, that in spite of all the aids now at the disposal of scientific men the riddle of the sun is yet unread. We have done so much, and gleaned so many facts at distances the very mention of which is almost meaningless to us, so stupendous are they, that we forget that our mighty sun, in spite of its brilliant shining and fostering heat, is still some ninety odd millions of miles removed; that its diameter is a hundred times greater than that of our earth; and that the yawning chasm we modestly call a sun-spot is yet large enough to swallow us up and half-a-dozen of our sister planets besides; while, if we employ the finest telescope under the most favourable atmospheric conditions, we are only enabled to observe the various phenomena as we should do with the naked eye at a distance of 180,000 miles.

Surely our powerlessness to grasp the physics of the sun should surprise no one, if this fact alone be borne in mind, while the "willow-leaf controversy" shows us the difficulty our observers have, not in collecting facts, but in actually being sure of what they see. The very stupendousness of the scale on which the action is carried on on the sun's surface is another difficulty, although a fortunate one, for without it we should see nothing. Still our observers, our physicists, and our theorists are not dismayed; and as a sign of the present activity, the question of solar physics has recently occupied the attention of our Royal Society and the French Academy of Sciences; a searching reduction of the Kew pictures and a notice of the physical aspect of the sun being presented to the former body by Messrs. De la Rue, Stewart, Loewy,[1] and Professor Phillips, and two papers dealing with the physical constitution of the sun being presented to the latter by M. Faye.[2]

It is to the communications to the last-named body that we now wish to draw attention; the more so as the part of it already published contains a very admirable historical notice of the progress of the inquiry. In a subsequent chapter we will discuss opinions at present held by our leading men of science.

Herschel's cloudy stratum.

M. Faye's historical notice commences by a reference to Wilson's theory of a solid, dark, and relatively cold nucleus and brilliant envelope, which (as already stated) was afterwards adopted by Sir William Herschel, who further embellished it with a cloudy stratum, lying between the nucleus and the photosphere. This stratum was eminently reflective, though not self-luminous, like the photosphere. Wilson's gaseous eruption, the bursting of which through the photosphere caused a "sun-spot," was now held to break through the cloudy stratum as well, and this latter was held to give rise to the appearance of the penumbra. It will be seen that both Wilson and Herschel built up their solar theory on terrestrial models. The latter went so far as to imagine in our auroræ a resemblance to the photosphere, and among the functions which he attributed to the cloudy stratum was that of rendering our sun a habitable globe. Hence the perfect reflecting power which he ascribed to it.

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[1] The results of the Kew work will be stated in detail subsequently.

[2] *Comptes Rendus*, vol. lx. pp. 89 and 138, 1865.

M. Faye, however, points out that, in addition to the theories now exploded, we owe to Wilson our first knowledge of two important facts: (1) That the spots are cavities; (2) That the photosphere is neither solid nor liquid, but of a nebulous, gaseous structure. On the first point the contrary opinions held by La Hire, Lalande, and some physicists of our own time are strongly commented upon, astonishment is expressed at the hardihood of those who hold them, and the beautiful stereoscopic combination imagined by De la Rue is advanced in support of the unanimous testimony of all who have ever observed the sun through a telescope, or have brought the laws of perspective to bear upon the inquiry. It is curious, however, that the crucial remark of Dawes, that if the spots were clouds we should have a notched limb whenever they passed off the disc, is not alluded to.

If the spots are clouds, we should have a notched limb when they leave or enter the sun.

On the second point, regarding the photosphere, it is remarked that astronomers would sooner have asserted the small mean density of the sun and its enormous heat in support of the evidence of their telescopes, if they had not so long held to the theory of the cool and habitable globe underneath. So that Arago's deduction from his experiments on the polarization of the sun's light—a deduction which supported the theory of the gaseous nature of the photosphere from a new point of view—was doubly welcome. Many objections were raised against the validity of this new argument. Some objected to it as being merely a cabinet experiment (we know better than this now); others have met it with absolute and complete denial. Thus, Sir John Herschel has stated that, in consequence of the enormous rugosity of the sun, the rays which reach us from any portion near the limb are not necessarily rays which leave the sun at a small angle from the light-giving surface; they may come from surfaces having all imaginable inclinations to the ray. It is evident, therefore, that the light coming from the margin contains a mixture of rays polarized in every direction,—a condition of things

Effect of the rugosity of the sun.

which would entirely vitiate Arago's conclusions. M. Faye thinks that if the illustrious "perpetual secretary" were still among us, he would reply, that at so great a distance a portion of the surface of any size, near the limb, would, in spite of these irregularities, affect a general direction, which would coincide with the mean surface to which is due the contour of the sun; hence a general predominance of obliquity in a given direction for the generality of rays which enter the polariscope: consequently the rays should contain a certain proportion of light polarized perpendicularly to the plane of convergence, if the radiating body is solid or liquid. M. Faye states, in addition, that he has lately been experimenting on globes of frosted silver, which everybody will grant presents a rougher surface, relatively, than the sun. Still the polarization was very strongly manifested towards the borders, and even on the parts much nearer the centre.

Faye's experiments on frosted silver globes.

A second objection is more important. Ångström has shown that gases and vapours absorb the rays of a refrangibility identical with that of the light which they themselves emit when brought to a state of incandescence; and, taking advantage of this discovery, MM. Kirchhoff and Bunsen have shown that it is possible to reproduce, artificially, the principal lines of the solar spectrum by interposing the vapours of various metals between a substance capable of giving out a continuous spectrum, and the eye.

Absorption of light by gases.

M. Kirchhoff has applied to the sun itself this excellent laboratory experiment. We want a source of light giving a continuous spectrum: this will be the photosphere. We want interposed metallic vapours: they will form the invisible atmosphere of the sun. The nature of these vapours will be determined by the solar rays absorbed, but incandescent solids and liquids give a continuous spectrum, whilst gases and vapours furnish only one limited to a few bright bands: therefore the photosphere, far from being gaseous, as we have thought, and as Arago believed himself

Kirchhoff's work.

CHAP. IV.

to have shown experimentally, must be a solid, because it gives a continuous spectrum. So these two celebrated experiments are contradictory; the polariscope says one thing, and the spectroscope says another, and many philosophers, forgetting the once vaunted experiment of Arago, have accepted the evidence furnished by the spectroscope.

Kirchhoff's hypothesis wrong.

M. Faye affirms that this fundamental contradiction is one in appearance only, and he undertakes to prove this in the second part of his memoir. In the interim he affirms again that M. Kirchhoff's conception of a solid or liquid incandescent sun cannot by any possibility represent the actual facts.

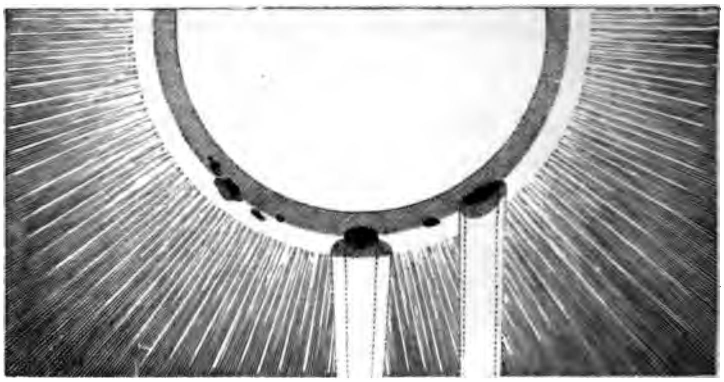

FIG. 20.—Explanation of Sun-spots on Kirchhoff's hypothesis that the spots are clouds and the photosphere is a liquid sea.

He remarks that if an actual solid or liquid condition be assigned to the photosphere, we must look elsewhere for the cause of the spots. This is what M. Kirchhoff has done, reproducing Galileo's first impression, in spite of Galileo's recantation of it. Galileo reasoned as follows: If two neighbouring spots are observed near the centre of the disc, with a bright interval between them, if the spots were protuberances this line of light would decrease as the spots approached the limb, and would soon disappear, because one of the spots, if it were a projection, would hide it. But observation proves that this line of light remains constant till the spots disappear. For the last two centuries and a half astronomers have contemplated and measured without finding Galileo's remark wanting, and it is useless to remark that Wilson's argument, unknown to Galileo, has finally fixed our ideas as to the nature of the spots.

Distribution of spots.

M. Faye next refers to the distribution of the spots on the sun's surface. Since the time of Fabricius, Galileo, and Father Scheiner even, we have known that the spots are confined to two zones between the parallels of 30° and 35° north and south, excepting an equatorial zone a few degrees wide, in which they very rarely show themselves; hence the spots are in relation with the sun's rotation, and we have analogous phenomena in the case of the larger planets, our own earth included.

Spots produced by descending currents.

Now if, as is the case with the planets, the polar regions of the sun are colder than the equatorial regions, there would be produced in its atmosphere—always accepting the theory of the dark nucleus—currents analogous to our trade winds, and consequently cyclones capable of tearing the photosphere, and even the cloudy stratum. On this hypothesis we can understand the narrow limits within which the spots appear, reasoning by analogy from the monsoon regions of the earth, which are those infested by cyclones; and further sun-spots would be produced by descending currents, and not by ascending eruptions, as held by Wilson and Sir William Herschel. Sir John Herschel's beautiful theory, that the sun is actually colder at the poles, by reason of the smaller thickness of the atmosphere in the polar regions allowing a greater radiation of heat, is therefore next alluded to, and it is remarked that this brilliant conjecture has at once taken the sun-spot theory out of the domain of perspective, and rendered it capable of dynamical treatment. Still, M. Faye does not accept Sir John Herschel's suggestion: he holds, in the first place, that the rotation of the sun is too slow to produce the atmospheric flattening required by it; and secondly, CHAP. IV. that the spots would affect, like our own clouds, a common movement of translation from the poles to the equator,—a movement not confirmed by modern observation. After alluding to the work of Mayer, Thomson, and others,—work which has enlarged the circle of our ideas, and banished from science the notion of a solar nucleus, dark and cool,—M. Faye promises in the next part of his paper to give the principal results of our modern work, and co-ordinate them, starting with the idea of a gradual cooling of an enormous mass, animated by a movement of rotation, and of an excessive temperature which maintains all the elements in a chaos of complete disunion, *except at the limit which separates the mass from the vacuum and cold of space.*

CHAP. V.

M. FAYE'S FIRST THEORY OF THE PHYSICAL CONSTITUTION OF THE SUN (continued).

WE said in the preceding chapter that the riddle of the sun was yet unread. A careful perusal of the second part of M. Faye's very remarkable memoir would almost make us pause before we repeated the remark, not because we have accepted his solution—it is not to be disposed of so easily—but because it is one of the most important contributions to this subject that we have had for some time, and it is quite possible that the full discussion which it certainly will receive at the hands of our physicists may result in an extension of our present knowledge, and a firmer standpoint for further investigations. The second part of M. Faye's memoir commences with a reference to the work done on the sun since Arago's time, that work being classed as follows:—(1) Study of spots, faculæ, and general surface of the photosphere. (2) The movement of rotation. (3) Phenomena exterior to the photosphere, observed during total eclipses of the sun. The latter are not referred to at any great length, as the photosphere is now in question principally, but it is shown that the presence of rose-coloured prominences on those parts of the sun where spots are never seen is a sufficient proof of the want of connection between the phenomena. Schwabe's sun-spot period—the lengthening of the spots in the direction of the parallel—the slow gyratory motion observed by Dawes—the obser-

Gyratory movement of spots.

vations of M. Chacornac—and the proofs brought forward by Dawes, Secchi, and De la Rue of the height of the faculæ, are mentioned, and reference is made to the work at Kew; and here, before we proceed, we will give, as briefly as possible, the latest results obtained by the Kew observers, which were communicated to the Royal Society at its last meeting,—results which bear closely upon this part of M. Faye's memoir. The materials at their disposal consisted of Mr. Carrington's original drawings from November 1853 to March 1861, and the Kew photographs taken continuously since 1858. To Nature as exhibited in these drawings they have put the questions:—

(1) Is the umbra of a spot nearer the sun's centre than the penumbra; or, in other words, is it at a lower level?

(2) Is the photosphere of our luminary to be viewed as composed of heavy solid or liquid matter, or is it of the nature either of a gas or cloud?

(3) Is a spot (including both umbra and penumbra) a phenomenon which takes place beneath the level of the sun's photosphere or above it?

Now if the umbra is appreciably at a lower level than the penumbra, we are entitled to look for an apparent encroachment of the umbra upon the penumbra on that side which is nearest the visual centre of the disk. This is the phenomenon observed by Wilson.

Encroachment of the umbra.

Taking all the cases where an encroaching behaviour of the umbra in a right and left direction has been perceptible, 86 per cent. are in favour of the hypothesis that the umbra is nearer the centre than the penumbra, while 14 per cent. are against it. Taking *all available spots* and distributing them into zones according to their distance from the centre, this encroaching behaviour is greatest when spots are near the border, and least when they are near the centre. Dealing with spots in high latitude only, and taking all those cases where an encroaching behaviour of the umbra in an up-and-down direction has been perceptible, 80·9 per cent. are in favour of the hypothesis that the umbra is nearer the centre than the penumbra, while 19·1 per cent. are against it.

Here, then, we find the most recent discussion of these observations bearing out entirely the view held all along by astronomers, and adopted by M. Faye in the first part of his memoir. Mr. Carrington's recent book is the principal text chosen by M. Faye on which to base his remarks on the rotation. He gives a table showing the period of rotation determined from observations of spots in both north and south heliocentric latitudes up to 50°,—a period varying from 25·06 days to 28·46. The displacement of the spots in latitude is especially dwelt upon, as they show that the great currents which on our globe transport masses of air from the poles to the equator do not exist on the sun,—a fact before referred to, and which tells equally against Herschel's and Kirchhoff's hypothesis. In fact clouds or cyclones cannot make head against the rotation with a rapidity of 2,000 leagues a day (in lat. 35°), except they approach the equator with a rapidity comparable to that of their movement in longitude. Now, if Mr. Carrington's observations tend to enlighten us at all on this head, past the 15th degree of latitude, they testify to a movement from the equator to the poles in exactly the opposite direction to that required by these hypotheses.

Transport of gas from poles to equator does not occur on the sun.

M. Faye then proceeds to give his explanation of the physics of the sun, taking as a start-point the fact that nothing really distinguishes our sun from the multitude of stars which shine in the heavens. Astronomers admit willingly that he is a star of mean size, of nearly white light, and slightly variable. We have, therefore, to deal with a phenomenon of considerable importance to us doubtless, but one after all of common, and indeed ordinary character, met with in the stellar universe. It is convenient, therefore, that we should set out with the most simple and general idea, and the one most applicable to the stars taken as a whole. This idea is that of the successive reunion in vast agglomerations, under the influence of attraction, of the matter of the materials first scattered through space. Hence two immediate consequences—(1) The destruction of an enormous quantity of *vis viva*, replaced by an enormous development of heat; (2) a movement of rotation more or less slow for the entire mass. The calculation of the amount of heat developed in the formation of the sun has been made by M. Helmholtz, by the aid of several plausible suppositions as to the numerical elements of the question. This calculation shows that it is easy to account for a duration of several millions of years, although chemical action would only sustain the present radiation of heat for half the actual historical period (3,000 years).

Chemical action is incompetent to maintain the sun's temperature.

This internal heat, when it is a question of masses so considerable, much surpasses that at which chemical action commences, but the cooling down in such a mixture of gases and vapours determines successive phases which M. Faye examines. In consequence of this cooling, in which conductibility exercises but an insignificant power by means of interior movements, a stable equilibrium between the different strata, analogous to that in our own atmosphere, in which the displacements are due to an exterior cause, will soon be established. Now, whatever may be the temperature of this homogeneous gaseous mass, its radiating power, confined to its surface—since each stratum possesses a special absorptive power for the rays emitted by the inferior layers—will be very small. The conducting powers being also fuller, the equilibrium of the entire mass will undergo but slow modifications, and unless new conditions be introduced, we cannot understand how such a mass can radiate the enormous quantity of heat, which seems to undergo no diminution in the course of ages. On this point M. Faye reasons as follows: "In fact, the temperature of the surface of the sun is far from being so elevated as that interior one of complete dissociation[1] just mentioned. From M. Pouillet's measures of the actual intensity of

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[1] A term borrowed from M. H. Sainte-Claire Deville.

Actual temperature of the sun.

solar radiation, Professor Thomson has found that the heat emitted is but from fifteen to forty-five times greater than that obtained in the furnaces of our locomotives. So that the superficial temperature will not enormously surpass that which we can produce in our laboratories,—a temperature, it is true, capable of producing the dissociation of a great number of substances, but which is still resisted by the more stable compounds.[1] The comparison of the light of the sun with that of our most powerful artificial light-sources corroborates this deduction.

"It results from this that, if the molecular and atomic forces of cohesion and affinity cease to act in the interior of the mass, they come into play on the surface, where, in a gaseous mixture of the most varied elements, the operations of these forces will give rise to precipitations (Herschel), clouds (Wilson), non-gaseous particles capable of incandescence, of which our brilliant terrestrial flames offer so many examples.[2] Soon these particles, obeying the forces of gravity, will in falling regain the temperature of dissociation, and will be replaced in the superficial layer by ascending gaseous masses, which will act in the same manner. The general equilibrium, therefore, will be disturbed in the vertical direction only by an unceasing exchange going on between the interior and the exterior—an exchange which was impossible in the preceding phase; and as the interior mass thus placed in connection with the exterior is so enormous, we can conceive that the superficial radiation, fed incessantly by the vast reservoir of central heat, constitutes a phase of long duration and of great constancy.

"Thus the formation of a photosphere—an apparent limit of the sun—is a simple consequence of cooling; and as our assumption applies to all analogous bodies, the same phenomena must exist, or must have existed, in all the stars.

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[1] This question will be discussed in the sequel; it is sufficient to state here that it is most probable that there is complete dissociation in the sun's atmosphere. (Note added 1873.)

[2] Chemical action in a gaseous mixture may be set up in two ways; by cooling if the mixture is at the temperature of dissociation, and by heat if the mixture is at an ordinary temperature.

Arago's and Kirchhoff's experiments reconciled.

"From this point of view the beautiful experiments of Arago and Kirchhoff are seen to be no longer contradictory. The term *incandescent gas* was not used by Arago in the sense attributed to it now. The flame he used was that of an ordinary gas jet, and not the *obscure* one of a Bunsen's burner, or of a simple gas. Even the numerous *savants* who now admit, on the authority of a name justly illustrious, that the sun and the stars have liquid photospheres, have not perhaps considered that incandescent molecules diffused in a gaseous medium, itself heated to a high temperature, give a continuous spectrum, with the exception of the dark lines due to the absorption of the medium.

Spectrum of limb and centre of disc identical.

"On the one hand, Arago's experiment conducts us to a correct conclusion, for light emitted from incandescent particles floating singly in a gaseous medium can only be natural light, from whatever depth it comes, because it undergoes, at no incidence, sensible refraction by the surrounding medium. On the other hand, this medium exercises its absorptive powers, and determines in the continuous spectrum of the incandescent clouds the system of lines which belongs to its complex nature. Looking at it in this way, we can understand why the spectrum of the limb is identical with that of the centre (a fact first advanced by Forbes, and recently confirmed by Janssen after a much more detailed study[1]), a result which would certainly not occur if all the black lines of the solar spectrum arose exclusively from the interposition of the gaseous strata of the general medium, which may extend beyond the photosphere to a height still unknown.

"The formation of the photosphere will now enable us to account for the spots and their movements. We have seen that the successive layers are constantly traversed by vertical currents, both ascending and descending. In this perpetual agitation we can readily imagine that *where the ascending current becomes more intense, the luminous matter of the photosphere is momentarily dissipated.* Through this kind of unveiling it is not the solid cold and black nucleus of the sun that we shall perceive, but the internal ambient gaseous mass, of which the radiating power, at the temperature of the most vivid incandescence, is so feeble in comparison to that of the luminous cloud of the non-gaseous particles, that the differences of these powers suffices to explain the contrast, so striking, of the two tones observed through our dark glasses."[1]

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[1] This point will be returned to.

Nucleus of a spot cooler than the photosphere.

M. Faye, after referring to a similar opinion held by Father Secchi, whose thermo-electric measures of the spots have shown that the nucleus of the spot radiates less heat than the photosphere, goes on :—

"But the most important phenomenon is assuredly that which has been so fully put in evidence by MM. Laugier and Carrington. Let us follow the same course of reasoning. From the continual exchange going on between the lower beds of the surface by means of vertical currents, we must conclude that the ordinary laws of rotation in a fluid mass in a state of equilibrium are strangely altered, since this equilibrium is constantly disturbed in a vertical direction. The ascending masses, which spring from a great depth, arrive at the top with a linear velocity of rotation less than that of the surface, because the layers whence they are derived have a smaller radius. Hence a general lagging in the

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[1] Is this in accordance with the received theory of radiation? (Note added 1865.)

This note was added to the article as it originally appeared, as the result of a conversation with my friend Dr. Balfour Stewart. I am the more anxious to state this, as to him belongs the credit of the objection, although, as it was some time afterwards put forward by Kirchhoff, the latter is now credited with it, although it was noticed by Faye, *Comptes Rendus*, vol. lxiii. p. 235, 1866. The idea is this :— If the interior solar gases are feeble radiators, then, on the theory of exchanges, they must be feeble absorbers ; hence they will be incompetent to absorb the light coming through the hypothetically gaseous sun from the photosphere on the other side. (1873.)

Motion of superficial currents in the sun.

movement of the photosphere, although this lag is compensated for the whole mass by the descending currents in such a manner that the fundamental law of areas is satisfied. In the same way our own atmosphere does not exactly follow the law of rotation of a mass in equilibrium, but the effects are all different because it rests upon a solid (or liquid) surface. If the photosphere lags behind the general rotation, the lower beds should, by way of compensation, be in advance of this movement. From this opposition it results that, although the photosphere will have a feeble tendency to approach the axis of rotation by flowing superficially towards the poles, the contrary tendency will be apparent in the lower strata, which will approach the equator. The phenomena will take place as if the start-points of the vertical currents belonged to an interior surface farther from the poles than from the equator; and if this ideal surface of emission be spheroidal, for instance, its depth, and consequently the retardation of the photosphere in successive zones, would vary nearly as the sine squared of the latitude. Now this is exactly what Mr. Carrington's empirical formula would give if the breach of continuity, to which M. Babinet has justly objected, were corrected by replacing the power $\frac{7}{4}$ of the sine, by $\frac{8}{4}$ or 2.[1] I find, in fact, that the observations are as well represented by the formula :—

$$\text{Daily motion} = 862' - 186' \sin^2 l.$$

" Here, however, facts cease to guide us ; the law of these variations is not really known. The variety of spots in the first 5° of the equatorial zone, and in the polar zone, which commences at 35°, does not permit us to determine at present the algebraical form of this variation. Here, then, is the problem which Mr. Carrington has bequeathed to us, and which we must at once attack with all the resources of science. It is to this part of the theory that will henceforth attach the distribution of spots, the phenomena of their periodicity, and the slight difference of temperature which seems to exist between the poles and the equator. . . . .

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[1] *Comptes Rendus*, Sept. 12, 1864, p. 481.

"As to the faculæ—luminous ridges which invariably indicate the near formation of a spot—*they are evidently due, like the spots, to ascending currents.* The photosphere is not a level surface in a mathematical sense; it is the limit to which the ascending currents transport, in the general fluid mass, the physical or chemical phenomena of incandescence. But although the phenomenon, taken as a whole, affects a remarkable regularity, since the brilliant surface appears to us perfectly spherical, we can imagine that a more than ordinary rapid local afflux may exceed this limit, and carry the luminous clouds a little above the general level. Hence the inequalities cited by Sir J. Herschel in his objections to Arago's experiment—inequalities confined, like the spots, to certain regions. From the fact alone that faculæ are elevated to a greater height in the general medium, their movement ought to be somewhat slower than the corresponding zone of photosphere; hence a tendency to appear to follow the spots—that is, to be to the left of them—than to incline over the spots when the impulse of the local current has ceased, and leaves the spots themselves to disappear in the rapid inrush of photospheric clouds."

Faculæ higher than the surrounding photosphere.

We will here, before we print the summary of M. Faye's memoir, give another extract from a paper recently read at the Royal Society. It will show that the most rigid reduction by De la Rue, Stewart, and Loewy, of the best observations that we possess, most of them taken by the sun himself, tends exactly to the results just stated by M. Faye:—

"The authors next endeavoured to answer the following questions:—Is the photosphere of our luminary to be viewed as composed of heavy solid or liquid matter, or is it of the nature either of a gas or cloud? It was observed that the great relative brightness of faculæ near the limb leads to the belief that these masses exist at a high elevation in the solar atmosphere, thereby escaping a great part of the absorptive influence which is particularly strong round the border; and this conclusion was confirmed by certain stereoscopic pictures produced by Mr. De la Rue, in which the faculæ appeared greatly elevated. It was remarked that faculæ often retained the same appearance for several days, as if their matter were capable of remaining suspended for a time; . . . . but of 1,137 cases, 584 have their faculæ entirely or mostly on the left side, 508 have it nearly equal on both sides, while only 45 have it mostly to the right. It would thus appear as if the luminous matter, being thrown up into a region of greater absolute velocity of rotation, fell behind to the left; and we have thus reason to suppose that the faculous matter which accompanies a spot is abstracted from that very portion of the sun's surface which contains the spot, and which has in this manner been robbed of its luminosity.

Faculous matter taken from the place occupied by the spot.

"Again, there are a good many cases in which a spot breaks up in the following manner:—A bridge of luminous matter of the same apparent luminosity as the surrounding photosphere, appears to cross the umbra of a spot unaccompanied by any penumbra. There is good reason to think that this bridge is above the spot; for were the umbra an opaque cloud, and the penumbra a semi-opaque cloud, both being above the sun's photosphere, it is unlikely that the spot would break up in such a manner that the observer should not perceive some penumbra accompanying the luminous bridge. Finally, detached portions of luminous matter sometimes appear to move across a spot without producing any permanent alteration.

"From all this it was inferred that the luminous photosphere is not to be viewed as composed of heavy solid or liquid matter, but is rather of the nature either of a gas or cloud; and also that a spot is a phenomenon existing below the level of the sun's photosphere.

"The paper concluded with theoretical considerations more or less probable. Since the central or bottom part of a spot is much less luminous than the sun's photosphere, it may perhaps be concluded that the spot is of a lower temperature than the photosphere. . . . .

"Finally, the authors propose the following question:—May not the falling behind of faculæ be the physical reaction of the proper motion of spots observed by Carrington, so that, while the current passing upwards falls behind, carrying the luminous matter with it, the current coming down moves forward, carrying the spot with it; *and may not this current coming from a colder region account for the deficient luminosity which characterizes a spot?*"[1]

Here we have the fundamental point of difference between M. Faye and the Kew Observers. We will now give the conclusion of M. Faye's memoir, which gives a *résumé* of the three middle phases of sun-life.

The three middle phases of sun-life.

(1) The *phase of complete dissociation* (that of the planetary nebulæ?), in which the heat decreases from the centre to the circumference. This state is susceptible of a particular equilibrium; the radiating power is very feeble; the light is purely superficial, since that of deep strata is capable of absorption by those at the surface. The spectrum is probably reduced to a number of brilliant bands, separated by extensive dark intervals.[2]

(2) *Cooling of the external strata* to such an extent that the action of certain molecular affinities becomes possible; formation of a photosphere, a kind of superficial laboratory, which determines the apparent outline of the mass; considerable radiating power of light and heat. The emitted light comes from a considerable depth in the photosphere; the spectrum of the preceding phase is inverted; the light is not sensibly polarized at any angle of emergence. The enormous flux of heat from the photosphere is kept up at the expense of the whole mass, by the action of ascending and descending currents, which are established between the

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[1] It must not be forgotten that in his paper Faye considers a spot as the seat of an uprush. (1873.)

[2] This looks like divination, if we suppose M. Faye to be ignorant of Mr. Huggins' discovery. (Note added 1865.)

lower strata and the periphery—currents impossible in the preceding phase. This second phase should occupy a considerable period of time, and present a great fixity in its phenomena. If the photosphere is locally wanting, the light and heat are reduced in that part in the ratio of the radiating power of the photosphere and the general gaseous medium. The movement of rotation is not executed exactly "bodily," as in the preceding phase, in which the conditions of equilibrium were nearly those of a fluid mass: the surface moves more slowly than the entire mass, in consequence of the antagonism of the forces which disturb the equilibrium. The superficial phenomena reveal the character of the intermittence.

Phase preceding extinction.

(3) When, in consequence of cooling, the vertical currents begin to decline, when the entire mass successively contracts to a sufficient mean density, the photosphere becomes very thick, and takes at the surface a liquid or pasty, and finally a solid consistency. Then the communication with the central mass is intercepted; the cooling of this mass is effected only in consequence of the conductibility of the liquids more or less pasty: that of the liquid or solid crust makes rapid progress at the superficies; the rotation which has been accelerated is regulated, the phenomena of spots and faculæ have disappeared, and the figure is that of a fluid mass in equilibrium under the action of interior forces. The intensity of the radiation decreases rapidly: that emitted obliquely is strongly polarized; the preceding spectrum does not change sensibly in appearance, but it also presents dark lines due to the real atmosphere, which is henceforward distinct from the body of the sun itself. The spectrum of the limb differs notably from that of the centre in the number and darkness of its lines.

After this phase comes extinction: this is the geological phase.[1]

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[1] This analysis of M. Faye's memoir is referred to by him in a subsequent communication to the Paris Academy: *Comptes Rendus*, vol. lx. p. 468.

THE SUN AS A TYPE OF THE MATERIAL UNIVERSE.[1]

IT is not necessary for our present purpose either that we should re-open the discussion as to the real discoverer of the solar spots, or that we should attempt to realize the strange and overwhelming mixture of wonder and awe, not to say delight and terror, with which the announcement must have been received. Man with a wondrous "optick-tube" had at last dared to peer into the secrets of the sun, and had, all unconsciously, by so doing dealt a deathblow at the fundamental Aristotelian doctrine of the immutability and incorruptibility of the heavens. The secret had been surprised; the sun was no longer the exemplar of spotless purity.

It is not astonishing, therefore, that whether we regard Galileo or Fabricius or Scheiner as the real discoverer, the secret was kept for many months before either of them gave it to the world; or that the latter, a Jesuit, was only permitted by his ecclesiastical superior—who, so runs the record, remarked to him that he had read Aristotle's writings from end to end many times without finding any mention of solar spots—to publish his discovery at last under a *nom de plume*. These facts tell as strongly as anything can do of the mixed emotions of those pioneers in the field of solar research. The secret divulged, however, the Schoolman was soon merged in the investigator, and the problem was attacked with a closeness and ardour which are almost models for modern observers. Witness Galileo's first letter to Welser, the chief magistrate of Augsburg, dated May 4, 1612, and Scheiner's last to the same personage, dated July 25, 1612, under the signature of *Apelles latens post tabulam.*

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[1] A joint communication to *Macmillan's Magazine* by the author and Dr. Balfour Stewart, F.R.S., who has permitted me to include it here.—J. N. L.

It would seem, indeed, that everything which could be reaped by the instruments at their command was immediately garnered. The motion of the spots across the sun's disc from east to west; the period in which they performed a complete circuit; their changes from day to day; the fact that they appeared for the most part in two zones, one north, the other south of the equator,—are samples of the secrets which the sun was at once compelled to yield up. What the spots were *not* was a question amply and closely discussed both by Galileo and Scheiner; but as to what they *were*, agreement was more difficult: Galileo at one time declared for clouds in an invisible atmosphere of the sun, Scheiner for a density and opacity equal to that of the moon—in fact for planets separated from the sun's surface, and revolving round him like Mercury and Venus.

Galileo thinks the spots are clouds, Scheiner that they are planets.

From the time of Galileo to 1769, or during nearly a century and a half, our knowledge was not increased by any new fact of importance, although in 1630 Scheiner managed to write a book of 784 pages[1] on the work which had been done in the two decades which had then elapsed since the discovery. It is true that Delambre has declared that there is not matter in this ponderous folio for fifty pages, but we hold that Delambre's dictum is harsh to a degree, and that when he made it he had entirely left out of sight the conditions under which the book had been written.

In the year 1769 there was a very large spot visible upon the sun, and Dr. Wilson, of Glasgow, observed it very carefully, and demonstrated subsequently[1] that the spot was a *cavity*—a conclusion which, although combated by Lalande at once, and by others in quite recent times, maintained and still maintains its ground. He also showed that the surface of the sun was probably of a cloudy nature.

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[1] "Rosa Ursina, sive Sol ex admirando facularum et macularum suarum," &c.

Wilson, the author of the important observation to which we have just referred, was also the first to put forward an elaborate theory of the origin and nature of sun-spots which much influenced the subsequent work till quite recently. These theories, subsequently taken up by Bode and Sir William Herschel, possess, however, but an historical interest, and it is no part of our present purpose to enlarge upon them. It must suffice to say that they were based on the assumption that the sun itself was a habitable, cool, glade-bedeckt globe beneath the luminous atmosphere, and that the appearance of a sun-spot was due to a gaseous eruption breaking through the cloudy envelopes of the solid globe: while La Hire held that they were purely surface-phenomena, and Lalande, that they were actual elevations.

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From the description of the telescopic appearance of the sun given in Chapter II., it is obvious that the surface of the sun is *uneven*, and that change of form is perpetually going on: these are conditions impossible in either a liquid or solid surface, such as land or ocean, but possible in a surface of cloud or gas.

The cloud-like nature of the sun's surface follows, moreover, from the nature of the sun's light. This increase of our knowledge we owe to those immortal discoverers Kirchhoff and Bunsen, whose wonderful generalization of the results of spectrum analysis has given the present century a new fulcrum on which to move the great unknown by the lever of inquiry, and bring it into the light.

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[1] "Phil. Trans.," 1774. See *ante*, page 10.

Their beautiful discovery not only enables us to define the sun as the nearest star, and to detect some ten terrestrial elements as existing in a state of vapour in its surrounding, absorbing, and *therefore cooler*, atmosphere; but it enables us to state, as a proved fact, that the light of the sun proceeds from solid or liquid[1] particles in a state of intense incandescence or glowing heat.

Motion of spots.

We shall shortly have occasion to refer again to this method of research: the more recent work regarding the spots demands attention, however, beforehand in order that we may follow as much as possible the order of time. It has already been stated that the early observers detected that the apparent motion of the spots was due to the real motion of rotation of the sun. But this account we now know is not all the truth. In addition to this apparent motion they have a real motion of their own of such a nature that the nearer a spot is to the sun's equator the faster it travels; in fact the rate of this proper motion depends upon the latitude of the spot. This was one of the chief results deduced by Mr. Carrington from an elaborate daily investigation of the sun extending over six years,—a stupendous work, unsurpassed in the acumen and patience brought to the task, and rarely equalled in the results achieved.

This discovery of the proper motion of the spots at once explained the strange discrepancies in the time of the sun's rotation as given by different observers,—discrepancies so great that Delambre declared it was useless to continue observations.

Sun-spot period.

Mr. Carrington's work did not stand alone about this time. The great Schwabe had previously determined that if the spotted area were taken at any one time, its amount varied from year to year,—that is, that the spots themselves were periodical; having periods of maximum and periods of minimum, the interval between two maximum or minimum periods being about eleven years. The lamented Dawes and Father Secchi largely increased our knowledge of the solar surface, the latter determining specially that there was less heat radiated from a spot than from the general surface.

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[1] Or densely gaseous, according to Frankland's researches on hydrogen, and later work. (Note added 1873.)

Some time after Mr. Carrington's book appeared, M. Faye took up the question of solar physics with his usual elaborate treatment, and communicated to the Paris Academy of Sciences two papers of great value, in which, *inter alia,* he broached a new theory to account for the observed phenomena, and especially to explain the dark appearances presented by the spots.

No combinations can exist in the interior of the sun.

M. Faye regards the interior of the sun as consisting of the original nebula, from which our whole system has been slowly condensed, in a state of dissociation; that is, at such an intense heat that chemical combinations are impossible; and he looks upon the photosphere as the surface at which this heat is so acted upon by the cold of space as to allow chemical combinations and solid and liquid particles to exist. He goes on to remark that, if the molecular and atomic forces of cohesion and affinity cease to act in the interior of the mass, they come into play on the surface,[1] where, in a gaseous mixture of the most varied elements, the operations of these forces will give rise to precipitations (Herschel), clouds (Wilson), and non-gaseous particles capable of incandescence, of which our brilliant terrestrial flames offer so many examples. These particles, obeying the force of gravity, will, in falling, regain the temperature of dissociation, and will be replaced in the superficial layer by ascending gaseous masses, which will act in the same manner. The general equilibrium, therefore, will be disturbed in the vertical direction only by an unceasing exchange going on between the interior and the exterior.

Having in this manner accounted for the photosphere and for the incessant change which is observed, M. Faye goes on as we translate him:—

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[1] I shall show in the sequel, from my own researches, that this is most probably not the case. (1873.)

"The formation of the photosphere will now enable us to account for the spots and their movements. We have seen that the successive layers are constantly traversed by vertical currents, both ascending and descending. In this perpetual agitation we can readily imagine that *where the ascending current becomes more intense the luminous matter of the photosphere is momentarily dissipated.* Through this kind of unveiling it is not the solid cold and black nucleus of the sun that we shall perceive, but the internal ambient, gaseous mass."

Faye states that the spots are uprushes;

In this quotation we have the two most important points of M. Faye's theory; namely, that the spots are caused by an uprush, and that their dark appearance is due to feeble radiation from a gaseous surface.

The faculæ also.

M. Faye also considers that the faculæ, like the spots, are due to ascending currents, and he then attempts to account for the proper motion of the spots by the ascending currents:—"From the continual changes going on between the lower beds of the surface by means of vertical currents, we must conclude that the ordinary laws of rotation in a fluid mass in a state of equilibrium are strangely altered, since this equilibrium is constantly disturbed in a vertical direction. The ascending masses which spring from a great depth arrive at the top with a linear velocity of rotation less than that of the surface, because the layers whence they are derived have a smaller radius. Hence a general lagging in the movement of the photosphere."

These remarks of M. Faye will be found in the *Comptes Rendus* for 16th and 23d Jan. 1865.[1] During the same month, a paper[2] was read at the Royal Society, in which certain results derived from the photographs taken at

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[1] See also Chapters IV. and V.

[2] "Researches on Solar Physics." By Warren De la Rue, Balfour Stewart, and B. Loewy (Proc. Royal Society, vol. xv. p. 37).

Kew, and certain theories based therefrom, were discussed. We limit ourselves to the two most typical passages in this paper:—

"Since the central or bottom part of a spot is much less luminous than the sun's photosphere, it may perhaps be concluded that the spot is of a lower temperature than the photosphere. . . .

"May not the falling behind of faculæ" (ample evidence of which is given in the paper) "be the physical reaction of the proper motion of spots observed by Carrington? so that while the current passing upwards falls behind, carrying the luminous matter with it, the current coming down moves forward, carrying the spot with it; and may not this current coming from a colder region account for the deficient luminosity which characterizes a spot?"

Difference between M. Faye's theory, and that of the Kew Observers.

We see at once that on these points there is a perfectly clear issue between the two theories. M. Faye holds the spot to radiate feebly because it is *hotter*—in fact because it unfolds to us the interior of the sun in a state of dissociation. The Kew Observers hold that it is less luminous because it is *colder*. Again, M. Faye holds that a spot is due to an uprush: the Kew Observers, that it is due to a downrush.

At the outset there were many arguments against M. Faye's hypothesis. The law of exchanges was utterly against his idea of the darkness of a spot,[1] for if it were the interior of the sun which we saw, and its radiation were feeble, then its absorption would have been equally feeble and the sun would be spotless; for where the photosphere was torn away on the side nearest us, we should be able to see, *through the sun*, the lower surface of the photosphere on the opposite side.

Again, the arguments in favour of an uprush, in the case both of spots and faculæ, are not very clear, nor have we a satisfactory explanation of the falling behind of the faculæ. But we had not long to wait for facts which, as far as we can see, have entirely settled the question. First, as to the downrush into a spot. In 1865 two observers—one in France, the other in England—carefully observed the fine spots from time to time visible on the sun's disc in that year; and the observations of both tend to show the absolute certainty that if spots are not caused by downrushes, they are, at all events, fed by them.

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[1] See note on p. 57, and *Comptes Rendus*, vol. lxiii. p. 234, 1866.

Let us hear the French observer first:[1] "La rapidité des changements est telle, que l'on peut suivre dans une même journée des courants des matières photosphériques se précipitant dans le gouffre principal en y transportant les petites taches voisines; celles-ci en s'ajoutant à la grande, augmentent son ouverture et prouvent ainsi que la masse entière de cette portion de l'écorce solaire est transportée par ce courant."

Visual evidence as to downrush.

The evidence derived from a spot observed in the next month through London fog is not less conclusive.[2] The spot had a tongue of facula stretching half-way over it. When the observation commenced at 11.30 on April 2, this tongue of facula was extremely brilliant; by 1 o'clock it had become less brilliant than any portion of the penumbra: at the same time the faculous mass seemed to be giving out at its end, veiling the umbra gradually with a kind of stratus cloud evolved out of it, which after a time again condensed into masses resembling the willow-leaves in the penumbra, only less distinct.

The argument for the downrush is to be found in the fact of the diminution of brightness; accepting as proved, first, that the faculæ are higher than the general surface, and, secondly, that a spot is a cavity. But it does not wholly depend upon this, for the masses or granulations on the general surface of the sun appear to lengthen out when they reach the penumbral region, as if they were acted upon by a current, and this may also explain the constantly observed difference in the shape of the cloud masses on the general surface and in the penumbræ. In this connection it is worthy of remark, that when a solitary willow-leaf is seen over the centre of a spot,[1] it is often observed to be nearly circular, as if its longer axis were tipped down. It is fair to add, however, that observations of the requisite delicacy can be very rarely made, owing to the many coincident conditions necessary.

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[1] M. Chacornac, "Bulletin des Observations faites à Ville-Urbanne. Groupes des Taches Solaires," 6th March, 1865.

[2] "Observations of a Sun-Spot," by the author (Monthly Notices of the Royal Astronomical Society, vol. xxv. p. 236). See *ante*, pp. 24–31, and *Comptes Rendus*, vol. lxi. p. 397.

Spectroscopic evidence.

The fact that a spot is due to absorption has next to be considered. On M. Faye's theory, as it will doubtless have already suggested itself to the reader, could a sun-spot be observed by means of a spectroscope,—as, *by hypothesis*, we have radiation from a gas in a state of dissociation,—the resulting spectrum would be a gaseous one; that is, it would consist of bright lines. We, in fact, should get from a spot a spectrum absolutely different from that which belongs to the light emitted from the general surface, *the latter* being a band of rich colour going from red through yellow, green, blue, indigo, to the intensest lavender, crossed by innumerable black lines of different intensity, the former consisting only of three or four thin bands of light, located in the green portion of the spectrum.

On the absorption-hypothesis there would be none of these bright lines; we should get a spectrum in the particular region of the spot similar to the average solar one, but showing evidence of greater absorption. This was put to the test in 1866.[2]

The method adopted was to apply a direct-vision spectroscope to a 6¼-inch equatorial, so that it was possible to observe at one time the spectra of the umbra of a spot and of the adjoining photosphere or penumbra.

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[1] See *ante*, p. 28.

[2] "Spectroscopic Observations of the Sun," by the author (Proceedings of the Royal Society, vol. xv. p. 256). (This is given *in extenso* further on.)

On turning the telescope and spectrum-apparatus, driven by clock-work, on to the sun, the solar spectrum was observed in the field of view of the spectroscope with its central portion (corresponding to the diameter of the umbra falling on the slit) greatly enfeebled in brilliancy.

Selective absorption of spots intensified.

All the absorption-bands visible in the spectrum of the photosphere, above and below, were visible in the spectrum of the spot; *but they appeared thicker where they crossed the spot-spectrum.*[1] There was not the slightest indication of any bright bands.

The dispersive power of the spectroscope employed was not sufficient to enable it to be determined whether the decreased brilliancy of the spot-spectrum was due in any measure to a greater number of bands of absorption.

The Royal Society at once recognized the importance of this discovery, although it was put forward with much hesitation, as the instrument employed was not of sufficient dispersive power, and the spot itself was not a very favourable one for the experiment. A larger instrument has now been constructed, and detailed observations are now about to be commenced under the auspices of that body. In the meantime, however, this settlement of the long-debated question has recently been entirely endorsed by Mr. Huggins, whose discovery of the physical constitution of nebulæ, and spectroscopic observations of the fixed stars, make his opinion of the greatest possible weight.

We have thus, as briefly as possible, traced up our knowledge of the sun's surface from the times of Galileo to our own. That surface, we have learnt, is of a cloudy nature, the light and heat being derived from the solid incandescent particles of which the clouds are composed. Further, there are exchanges perpetually going on between the cooler exterior and the interior. The descending current is accompanied by a spot, the ascending one by a facula; and finally, the dark appearance of a spot, like the darkening of the limb, is due to the absorptive properties of the sun's atmosphere.

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[1] A diagram of the appearance observed when more dispersion is used than I then employed is given further on.

Let us, for one moment, compare the sun's envelope with our own, and observe the action of the latter when the sun is withdrawn.

The general surface of the ground is a good radiator. On the other hand, the atmosphere is at once a feeble absorbent and a feeble radiator. When the sun's influence is withdrawn from the earth's surface, and the sky is clear, the general surface of the ground and the leaves of plants give off their heat, which is radiated into space unimpeded by the very feeble absorbing power of the air; on the other hand, the air, being a feeble radiator, gives back little or nothing in return.

Analogy between solar and terrestrial radiative phenomena.

As far as radiation is concerned, therefore, the ground and leaves get rapidly cooler, nor is this loss of heat made up by any other process. Little or no heat can reach the cooled surface by conduction, for ground, leaves and air are bad conductors. Further, convection does not operate, for the particles of air next the cooled surface becoming cooler themselves become also heavier, and remain where they are. There is, therefore, no hindrance to the cooling of the earth's surface, which in its turn cools the air in contact with it until the air has reached so low a temperature that it cannot longer retain all its vapour. Part of the vapour is, therefore, deposited as moisture (or hoar frost if the temperature be below freezing-point), on the surface of the ground and the leaves of plants; and this is the explanation of dew and hoar frost, which we get when there is free exposure to the open sky. If there be cloud, a glass frame, matting, or any obstacle in the shape of a good radiator, interposed between the body and the sky, there will be no deposition of dew, because a quantity of heat will be derived from the radiator which has been interposed. Heat will therefore be lost very slowly, and moisture will not be deposited. It must be borne in mind that the presence of cloud makes an essential difference. We may suppose something equivalent to the deposition of dew, or at all events great radiation, to be taking place *on the upper surface of the cloud*, not *under the cloud*. The heat of the bodies is retained in the latter region, the radiation being diminished, or rather compensated, by counter-radiation. It may be instructive to place ourselves in imagination above the surface of such a cloud, the sun being withdrawn, and consider for a moment what probably takes place. The small deposited particles, being great radiators, will rapidly get colder than the surrounding air; they will, at the same time, cool the air around them; and the air, being cooled, and thereby rendered heavier, will descend. There *will thus be descending currents of air.* But descending convection currents are naturally accompanied with ascending ones. *There will therefore be ascending currents*, conveying upwards some of the comparatively warm air from below. It is not impossible that such currents may assume in nature somewhat large dimensions, and that the cloud may therefore present to a beholder regarding it from a great distance above, an irregular, pitted, notched shape; in fact, exactly such an appearance as we see on the sun, the envelope of which, *parvis componere magna*, may resemble in its mechanism that of a planet like our own with its sun withdrawn.

Circulation in radiating gaseous masses.

Eclipse phenomena.

So far we have only referred to the phenomenon ordinarily visible to us. Another part of the sun's physical constitution is rendered visible during total eclipses. We allude to the nature of its atmosphere. Eclipse-teachings, therefore, are of high value; but they certainly are not of such high value as ordinary observations of its surface, although they are in their nature much more sensational, for a total eclipse of the sun is at once one of the grandest and most awe-inspiring sights it is possible for man to witness. All nature conspires to make it strange and unearthly.

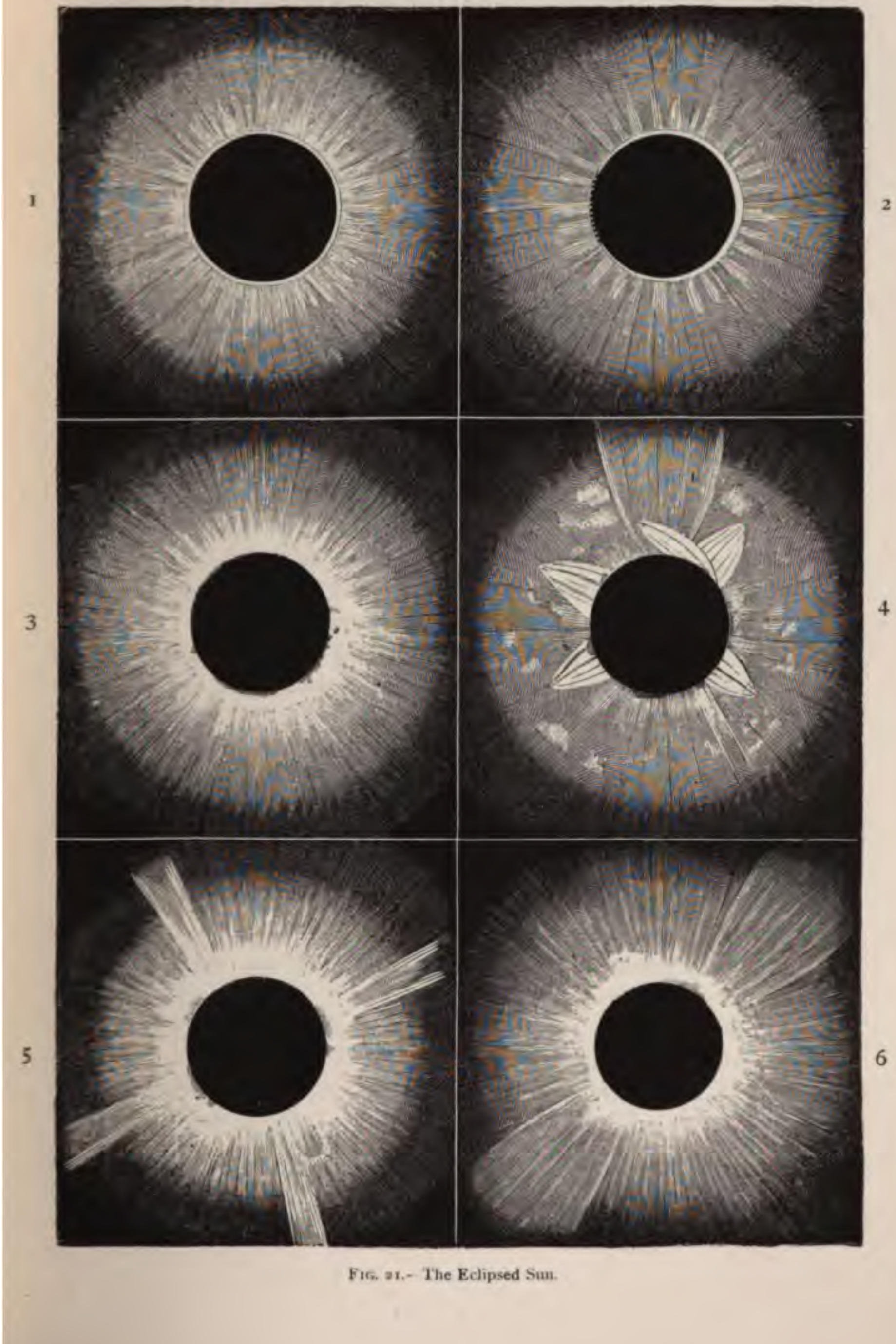

Fig. 21.— The Eclipsed Sun.

Soon the stars burst out; and surrounding the dark moon on all sides is seen a glorious halo, generally of a silver-white light: this is called the corona. It is slightly radiated in structure, and extends sometimes beyond the moon to a distance equal to her diameter. Besides this, rays of light, called aigrettes, diverge from the moon's edge, and appear to be shining through the light of the corona. In some eclipses parts of the corona have reached to a much greater distance from the moon's edge than in others.

The corona

It is supposed that the corona is the sun's atmosphere, which is not seen when the sun itself is visible, owing to the overpowering light of the latter.

When the totality has commenced, apparently close to the edge of the moon, and therefore within the corona, are observed fantastically-shaped masses, full lake-red, fading into rose-pink, variously called red-flames and red-prominences. Two of the most remarkable of these hitherto noticed were observed in the eclipse of 1851.

and prominences.

It has been definitely established by the exquisite eclipse photographs of De la Rue and Secchi, that these prominences belong to the sun, as those at first visible on the eastern side are gradually obscured by the moon, while those on the western are becoming more visible, owing to the moon's motion from west to east over the sun. The height of some of them above the sun's surface is upwards of 70,000 miles.

Their magnitude.

The plate on the previous page, of various solar eclipses, may serve to give an idea, at the best a poor one, of the extraordinary appearances which are then seen surrounding the dark moon. Thus, Fig. 1 represents the ordinary phenomena of an annular eclipse; Fig. 2, the annular eclipse of 15th May, 1836, showing Baily's beads; Fig. 3, the *total* eclipse of 28th July, 1851, as sketched by Dawes; Fig. 4, the total eclipse of 7th September, 1858, as observed by Liais; Fig. 5, the total eclipse of 18th July, 1860, according to Feilczsch; and lastly, the total eclipse of 8th July, 1840.

It is not yet known what these strange red prominences are; but while we write, astronomers are trooping to India to settle the question at the coming total eclipse. England, France, Prussia, and other European states will be represented, while—a happy evidence of the sooner or later prevalence of truth—the successor of Galileo's persecutor will be represented by one of the most accomplished astronomers of modern times—Father Secchi, a Jesuit, who, we trust, will be among the foremost to crown the edifice of which Galileo laid the foundation-stone;[1] for, in fact, a knowledge of the nature of the red prominences seems now to be the only thing wanting to complete a sketch of the visible solar phenomena apart from their causes. Of course there is much detailed drawing to be added afterwards.

But, even at present, we are in a position to imagine what the real nature of the prominences may be.

In the first place, a diligent spectroscope sweeping round the edge of the sun has not revealed any bright lines.[2] This is strong negative evidence that they are not masses of incandescent vapour or gas; for as the light from such vapour or gas is almost monochromatic, it should be as easy to detect as that of the immeasurably distant nebulæ.

The solar atmosphere is cooler than the photosphere.

Secondly, we know that the atmosphere of the sun is colder than the photosphere, and that in the latter we have incandescent particles of solid matter. As the prominences are possibly not due to incandescent vapour, the question remains whether they may be attributable to sub-incandescent particles of solid matter at a red glowing heat only, suspended in the atmosphere: in fact, whether the particles in the photosphere itself may not be likened to a white-hot poker, and those in the atmosphere to merely a red-hot one.

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[1] Father Secchi, however, did not go. (1873.)

[2] On this point it must be here remarked, that, until the experiments here referred to were made, it was the settled conviction both of Dr. Stewart and myself, that the prominences were masses of glowing gas. Some of the reasons for this opinion will be found in a communication by Dr. Stewart to the *Philosophical Magazine* in 1862 (vol. xxiv. p. 305), three years before the experiments were made. Others occur upon the surface: for instance, their great actinic power, combined with their colour, which could only point to a something with lines at both ends of the spectrum; their shapes and appearances; and their position above the mobile region of the spot level. See my communications to the Royal Society in 1866 and 1868, which will be given afterwards. (Note added 1873.)

In the previous part of this article, attention has been directed solely to the *immediate cause* of a sun-spot; and an attempt has been made to show that a downrush of comparatively cold atmosphere from above, accompanied with an uprush of warm atmosphere from below, is the only sufficient explanation of the phenomena observed. It has also been shown, as the result of a careful scrutiny of the whole surface of the sun, that there are probably convection currents in constant operation all over the disc —a condition of things which we might expect from the intensely hot state of the sun's surface combined with the enormous gravity of matter there placed. A sun-spot may thus not improbably be regarded as an enormous development under exceptional circumstances of what is constantly occurring all over the sun's surface. This remark brings us a step further in our inquiry by suggesting the question, What are the exceptional circumstances that cause the ordinary convection currents of the sun's surface to develop themselves occasionally into sun-spots? This inquiry may be rendered more general by dismissing from the mind all idea of the nature of sun-spots: it is not essential to know what they are, whether convection currents or something else. The question now is not what is their nature, but what is their cause, or rather, in the present state of our ignorance, are they connected with any other phenomena that may serve to throw light upon their cause? This inquiry divides itself into the four following heads:—

1. Does the amount of spotted surface of the sun vary from time to time?

2. Is the region of outbreak of a spot confined to any particular part of the sun's disc?

3. When a spot is formed, does it obey any laws with regard to increase and diminution?

4. And finally, are spots connected with any other phenomena on the earth's surface or elsewhere?

The remainder of the article will consist of an attempt to answer these four questions.

Sun-spot period of ten years.

Now, in the first place, as has been already noticed, the amount of spotted surface has a ten-yearly period. This has been discovered through the labours of the veteran astronomer Hofrath Schwabe, of Dessau, who has now for about forty years been engaged without intermission in registering the number of spots which appear on the sun's surface.

Herr Schwabe has found as the result of his labours, that in the year 1828 there were 225 groups, against 161 groups in 1827, and 199 in 1829; the year 1828 was therefore a year of maximum. After this the number of groups gradually decreased until in 1833 there were only 33 new groups observed. After this year they began again to increase, and in 1837 they attained another maximum. The next year of maximum was 1848, and the next after it 1859. We may therefore expect another in the course of a few years; indeed at the present moment the number of spots is increasing. We are still ignorant of the ultimate cause of this periodicity, but independent observations by the Kew Observers,[1] and by Hofrath Schwabe, lead to the impression that in years of minimum there is a less amount of cold-absorbing atmosphere above the photosphere, and consequently a smaller tendency to the downrush of cold matter in large quantity. The observations above referred to seem to indicate for years of minimum a more uniform brightness of the sun's surface, —that is to say, a less amount of absorption or falling off

Absorbing atmosphere less in the minimum period.

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[1] "Researches on Solar Physics." 1st and 2d Series. By Messrs. De la Rue, Stewart, and Loewy.

towards the limb, a phenomenon which, it has been already shown, depends upon the amount of cold-absorbing atmosphere above the region of light.

We pass on to the second question, as to the region of outbreak of a spot.

Spot zones.

This question has been answered in an admirable manner by Carrington, who showed in a complete discussion of all the spots extending from 1854 to 1860, that, generally speaking, the region of outbreak of spots is the equatorial zone of the sun. At certain periods, however, he has shown that the zone is very closely confined to the equator, though at other periods it opens out. Such an opening out began about September 1856, at which epoch the generality of spots were for the most part found at a latitude of 30° either north or south of the solar equator. After this they gradually narrowed in towards the equator. The date of the next widening out cannot be given until the Kew records are reduced, but it is believed that at the present moment, or very recently, there has been a similar phenomenon. Thus while, generally speaking, spots attach themselves to the equatorial region of the sun, they are nevertheless inconstant in their attachment; and just as we have a small ripple proceeding on the back of a large wave, so we have minor periods of opening out proceeding on the back of the large period described by Carrington. The Kew Observers have very recently described a smaller period of this kind, of four months as nearly as possible.

So much for the solar latitude of sun-spots, and now one word with regard to solar longitude. If the sun could be sliced like an orange from pole to pole by sections of longitude, it is conceivable that one of these sections might be found to be composed of a different material from the others, more favourable to the development of spots. As a matter of fact, however, we have no reason for supposing this; and we believe that the conclusion come to by Carrington as the result of his researches is, that there seems to be no continuous preference given to one solar longitude over another as far as regards the outbreak of spots. But this leads us on to the next question, as to the behaviour of a spot when once formed, with regard to increase and diminution.

No preference for any particular longitude for the outbreak of spots.

Now, while it may with much probability be asserted, that no continuous preference is shown to one solar longitude over another as regards the outbreak of spots, yet the longitudinal portion at which a spot breaks out, and its behaviour after it has made its appearance, are nevertheless not accidentally determined. It is an astounding but apparently well-proved fact, that the birth and behaviour of spots are regulated by the position of the planetary bodies, so that we may cast the horoscope of a sun-spot with some approach to truth. In order to obtain grounds for this conclusion, the Kew Observers have laboriously measured the area of all the sun-spots observed by Carrington from 1854 to 1860, and they find, as the result of their inquiries, that a spot has a tendency to break out at that portion of the sun which is nearest to the planet Venus. As the sun rotates, carrying the newly-born spot further away from this planet, the spot grows larger, attaining its maximum at the point furthest from Venus, and decreasing again on its approaching this planet. We here speak of Venus, as it appears to be the most influential of all the planets in this respect. Jupiter appears also to have much influence; and, more recently, it has been shown that Mercury has an influence of the same nature, although more difficult to discuss on account of his rapid motion.

Outbreak of spots.

Should therefore any two of these planets—or, still better, should all three—be acting together at the same place upon the sun, we may expect a very large amount of spots, which will attain their maximum at that portion of the sun most remote from these planets. When we say that very good evidence has been shown for this statement, we mean that it would have been reckoned conclusive had the statement been of a less wonderful character; and, as this conclusion is not less important than wonderful, we trust that these researches, which are being prosecuted under the auspices of the Royal Society, will be continued until the last remnant of doubt is removed from the mind of the most sceptical.

Connection of spots with terrestrial phenomena.

Finally, are spots connected with any other phenomena on the earth's surface or elsewhere? For an answer to this question we are mainly indebted to the labours of General Sabine, the present distinguished President of the Royal Society. General Sabine has shown, as the result of laborious and long-continued observations in various parts of the globe, that there are occasional disturbances in the magnetic state of the earth, and that these disturbances have a periodical variation coinciding in period and epoch with the variation in frequency and magnitude of the solar spots as observed by Schwabe; and the same philosopher has given us reason to conclude that there is a similar coincidence between the outburst of solar spots and of the Aurora Borealis.

Variation of sun's heat.

Very recently, also, Mr. Baxendell, of Manchester, has published some observations from which we may, perhaps, conclude that the direct heat of the sun's rays varies with the state of the sun's surface. These observations require confirmation, but they bear out the idea that at these periods there is a greater amount of cold-absorbing atmosphere above the sun's photosphere; that is to say, the photosphere is further down or nearer the sun's centre, and hence we may suppose of a somewhat higher temperature than when it is further up. Under this heading it may be stated that we believe Hofrath Schwabe conjectures the possibility of a periodicity in the appearance of the planet Jupiter, coinciding with the period of spot-frequency. This, however, is not yet proved.[1]

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[1] We shall subsequently show that terrestrial rainfall and cyclones are intimately connected with sun-spots.

We now give the following extract from the concluding remarks of the Kew Observers in their paper on Planetary Influence:—

"The following question may occur to our readers:—How is it possible that a planet so far from the sun as Venus or Jupiter can cause mechanical changes so vast as those which sun-spots exhibit? We would reply in the following terms to this objection:—

"We do not of course imagine that we have as yet determined the nature of the influence exerted by these planets on the sun; but we would nevertheless refer to an opinion expressed by Professor Tait, that the properties of a body, especially those with respect to heat and light, may be influenced by the neighbourhood of a large body. Now an influence of this kind would naturally be most powerful upon a body such as the sun, which possesses a very high temperature, just as a poker thrust into a hot furnace will create a greater disturbance of the heat than if thrust into a chamber very little hotter than itself. . . . . . The molecular state of the sun, just as that of the cannon or of fulminating powder, may be externally sensitive to impressions from without,—indeed, we have independent grounds for supposing that such is the case. We may infer from certain experiments, especially those of Caigniard de Latour, that at a very high temperature and under a very great pressure the latent heat of vaporization is very small, so that a comparatively small amount of heat will cause a considerable mass of liquid to assume the gaseous form, and *vice versâ*. We may thus very well suppose that an extremely small withdrawal of heat from the sun might cause a copious condensation; and this change of molecular state would, of course, by means of altered reflection, &c. alter to a considerable extent the distribution over the various particles of the sun's surface of an enormous quantity of heat, and great mechanical changes might very easily result."

Effects of small increments of heat on bodies at a very high temperature.

The speculative outcome of the investigation described in the latter part of this article may be briefly stated as follows:—

Intimate relations existing between the sun and planets.

There seems to be great molecular delicacy of construction in the sun, and probably also, to an inferior extent, in the various planets; and the bond between the sun and the various members of our system appears to be a more intimate one than has hitherto been imagined. *The result of all this will be that a disturbance from without is very easily communicated to our luminary, and that when it takes place it communicates a thrill to the very extremities of the system.*

In a future article the principle of delicacy of construction will be dwelt upon at greater length, more especially with reference to the Place of Life in a Universe of Energy.

THE PLACE OF LIFE IN A UNIVERSE OF ENERGY.[1]

THERE is often a striking likeness between principles which nevertheless belong to very different departments of knowledge. Each branch of the tree of knowledge bears its own precious fruit, and yet there is a unity in this variety—a community of type that prevails throughout. Nor is this resemblance a merely fanciful one, or one which the mind conjures up for its own amusement; while it has produced a very plentiful crop of analogies, allegories, parables, and proverbs, not always of the best kind, yet parables and proverbs are or ought to be not fictions but truths.

We shall venture to begin this article by instituting an analogy between the social and the physical world, in the hope that those more familiar with the former than with the latter may be led to clearly perceive what is meant by the word ENERGY in a strictly physical sense. Energy in the social world is well understood. When a man pursues his course undaunted by opposition, unappalled by obstacles, he is said to be a very energetic man. By his energy, we mean the power which he possesses of overcoming obstacles; and the amount of his energy is measured by the amount of obstacles which he can overcome, by the

Actual energy.

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[1] A joint communication to *Macmillan's Magazine* by the author and Dr. Balfour Stewart, F.R.S., who has permitted me to include it here.—J. N. L.

amount of work which he can do. Such a man may in truth be regarded as a social cannon-ball. By means of his energy of character he will scatter the ranks of his opponents and demolish their ramparts. Nevertheless such a man will sometimes be defeated by an opponent who does not possess a tithe of his personal energy. Now, why is this? The reason is that, although his opponent may be deficient in personal energy, yet he may possess more than an equivalent in the high position which he occupies, and it is simply this position that enables him to combat successfully with a man of much greater personal energy than himself. If two men throw stones at one another, one of whom stands on the top of a house and the other at the bottom, the man at the top of the house has evidently the advantage.

Energy of position.

So in like manner, if two men of equal personal energy contend together, the one who has the highest social position has the best chance of succeeding.

But this high position means energy under another form. It means that at some remote period a vast amount of personal energy was expended in raising the family into this high position. The founder of the family had doubtless greater energy than his fellow-men, and spent it in raising himself and his family into a position of advantage. The personal element may have long since vanished from the family, but it has been transmuted into something else, and it enables the present representative to accomplish a great deal, owing solely to the high position which he has acquired through the efforts of another. We thus see that in the social world we have what may be justly called two kinds of energy, namely—

1. Actual or personal energy.
2. Energy derived from position.

Let us now turn to the physical world. In this as in the social world, it is difficult to ascend. The force of gravity may be compared to that force which keeps a man down in the world.

If a stone be shot upwards with great velocity, it may be said to have in it a great deal of actual energy, because it has the power of overcoming the obstacle interposed by gravity to its ascent, just as a man of great energy has the power of overcoming obstacles.

This stone as it continues to mount upwards will do so with a gradually decreasing velocity, until at the summit of its flight all the actual energy with which it started has been spent in raising it against the force of gravity to this elevated position. It is now moving with no velocity, and may be supposed to be caught and lodged upon the top of a house.

Energy is not lost, but changed in form.

Here, then, it rests, without the slightest tendency to move, and we naturally inquire, What has become of the energy with which it began its flight? Has this energy disappeared from the universe without leaving behind it any equivalent? Is it lost for ever, and utterly wasted? Far from it; the actual energy with which the stone began its flight has no more disappeared from the universe of energy than the carbon which we burn in our fire disappears from the universe of matter.

It has only changed its form and disappeared as energy of actual motion in gaining for the stone a position of advantage with respect to the force of gravity.

Thus it is seen that during the upward flight of the stone its energy of actual motion has gradually become changed into energy of position, and the reverse will take place during its downward flight, if we now suppose it dislodged from the top of the house. In this latter case the energy of position with which it begins its downward flight is gradually converted into energy of actual motion, until at last, when it once more reaches the ground, it has the same amount of velocity, and therefore of actual energy, which it had at first.

Thus we have also in the physical world two kinds of energy: in the first place we have that of actual motion, and in the next we have that of position. We see from this how intimate is the analogy between the social and the physical worlds as regards energy, the only difference being that, while in the former it is impossible to measure energy with exactness, in the latter we can gauge it with the utmost precision, for it means the power of performing work, and work (it is needless to mention in this mechanical age) is capable of very accurate measurement.

There are several varieties of energy in the universe, and, Proteus-like, it is always changing its form. Had it not been for this habit, we should have understood it long since, but it was only when its endeavours to escape from the grasp of the experimentalist were of no avail, that it ceased its struggles and told us the truth.

Forms of energy.

All of these varieties may, however, be embraced under the two heads already mentioned,—namely, *energy of actual motion* and *energy of position.*

Energy of motion.

A railway train, a meteor, a mountain torrent, represent *energy of motion,* but there is also invisible molecular motion which does not the less exist because it is invisible. Such for example is heat, for we have reason to believe that the particles of hot bodies are in very violent motion. A ray of light is another example of energy of motion, and so likewise is a current of electricity; and if we associate the latter with a flash of lightning, it ought to be remembered that the flash is due to particles of air that have been intensely heated by electricity becoming changed into heat. Electricity in motion is pre-eminently a silent energy, and it is only when changed into something else that its character becomes violent.

Energy of position.

Then, again, as representing *energy of position* we may instance our stone at the top of the house, or a head of water, both of which derive their energy from their advantageous position with respect to gravity.

But there are other forces besides gravity. Thus a watch newly wound up is in a condition of visible advantage with respect to the force of the main-spring, and as it continues to go it gradually loses this energy of position, converting it into energy of motion. A cross-bow bent is likewise in a position of advantage with respect to the spring of the bow; and when its bolt is discharged, this energy of position is converted into energy of motion.

Invisible forms.

Besides this, there are invisible forms of energy of position. When we tear asunder a stone from the earth, and lodge the former on the top of a house, we obtain visible energy of position, the force *against* which we act being *gravity.* But we may also tear asunder from each other the component atoms of some chemical compound, our act here being performed *against* the very powerful force called *chemical affinity.*

Thus, taking a particle of carbonic acid, we may tear asunder the oxygen from the carbon, and, if our scale of operations be sufficiently great, we shall obtain separate from each other one mass of carbon and another of oxygen, —not, however, without the expenditure of a very large amount of energy in producing this separation.

We have, however, obtained a convenient form of energy of position as the result of our labours, which we may keep in store for any length of time, and finally, by allowing the carbon and oxygen to reunite,—that is to say, by burning the carbon,—we may recover in the shape of heat and light the energy which we originally expended in forcing these bodies asunder.

Some of the most prominent varieties of energy of motion and of position have now been described, and the remarks made have induced the belief that this thing, energy, this capacity which exists in matter for performing work of one kind or another, is by no means a fluctuating element of our universe, but has a reality and a permanence comparable to that which we associate with an atom of matter.

Conservation of energy.

The grand principle of the conservation of energy, a principle lately proved by Dr. Joule,[1] asserts that energy, like ordinary matter, is incapable of being either created or destroyed. We will endeavour to give two examples in illustration of this great law, which is worthy of the highest attention.

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[1] We ought not to omit the names of W. R. Grove and Mayer in connection with this generalization.

Rumford and Davy.

Let us first ask, with Rumford and Davy, When a hammer has struck an anvil, what becomes of the energy of the blow? or when a railway train in motion has been stopped by the brake, what becomes of the energy of the train? A proper understanding of what here takes place will very much conduce to a clear conception of the laws of energy.

Unquestionably in both these instances energy seems to have disappeared—to have vanished, at least, from that category which embraces visible energy, and we are taught to ask if the disappearance means annihilation or only a change of form. Let us examine what other phenomena accompany this seeming disappearance. It is well known that an anvil or piece of metal repeatedly struck by a hammer becomes hot, nay, even red hot, if the process be continued long enough. It is also known that when a railway train is stopped there is much friction at the brake-wheel, from which on a dark night sparks may be seen to issue. We may add to these the experiment of Davy, in which two pieces of ice are melted by being rubbed against each other. The concomitants of percussion and friction are thus seen to be in the first place an apparent destruction of energy, and in the second the apparent generation of heat; and this mere juxtaposition of the two phenomena is quite enough to suggest that in this case mechanical energy is changed into heat.

The second example to be mentioned in illustration of the laws of energy is the origin of coal or wood. Coal or wood, as we all know, is a very concentrated and convenient form of energy. We can bring a great deal of heat out of it, or we can make it do a great deal of mechanical work.

Now as wood grows, from whence does the wood derive its energy? We are entitled to ask this just as fairly as from what source it derives its particles. The wood, we answer, derives its energy from the sun's rays. Part of these rays is spent in decomposing carbonic acid in the leaves of plants, ejecting the oxygen (one of the products of this decomposition) into the air, but retaining the carbon in the leaf, and ultimately building up the woody fibre from this very carbon.

Nothing for nothing.

Nothing for nothing in these regions. The sun's energy is spent in producing the wood or coal, and the energy of the wood or coal is spent (far from economically, it is to be regretted) in warming our houses and in driving our engines.

These two illustrations will tend to impress upon the minds of our readers the truth of the grand principle of the conservation of energy.

Dissipation of energy.

The principle now described has reference, however, merely to quantity, and asserts that in all the various transmutations of energy there is no such thing as creation or annihilation. An additional principle discovered by Sir W. Thomson, and named by him the "dissipation of energy," refers to quality. And here also there is a striking analogy between the social and the physical world; for as in the social world there are forms of energy conducing to no useful result, so likewise in the physical world there are degraded forms of energy from which we can derive no benefit. And as in the social world a man may degrade his energy, so also in the physical world may energy be degraded; in both worlds, when degradation is once accomplished, a complete recovery would appear to be impossible, unless energy of a superior form be communicated from without.

The best representative of superior energy is mechanical effect. Another is heat of high temperature, or the means of producing this in the shape of fuel.

The mechanical energy of a machine in motion may not only give us useful work, but, if we choose, we can transmute it either directly or indirectly into all other forms of energy. Again, high-temperature heat is another very useful form of energy, and by means of the steam-engine it may be converted into mechanical effect. On the other hand, when heat is equally diffused or spread about, it represents the most degraded and worthless of all forms of energy. Nothing of value can be accomplished by its means. Thus, for instance, there is abundance of heat spread throughout the walls of the chamber in which we now write, but not a particle of all this can be converted into useful mechanical effect.

Long before any of these laws were known the superiority of certain kinds of energy was instinctively recognized; and desperate, but of course futile, efforts have ever and anon been made by enthusiastic visionaries to procure a perpetual motion or an ever-burning light. We could amuse our readers, if we had time, with some of these: the lesson they teach is that no ingenuity can raise a superstructure without foundations. The possibility of a perpetual motion still lingers in the minds of certain enthusiasts, but the idea of an ever-burning light has vanished long since; it seems more than the other to have been associated with pretensions to magic. Thus, in "The Lay of the Last Minstrel," we find the monk of St. Mary's Aisle describing in the following words the grave of the famous wizard Michael Scott:—

> "Lo, warrior! now the cross of red
> Points to the grave of the mighty dead;
> Within it burns a wondrous light,
> To chase the spirits that love the night.
> That lamp shall burn unquenchably,
> Until the eternal doom shall be."

Now the law of the dissipation of energy shows us at once why a perpetual motion and an ever-burning light are both equally impossible. It asserts that there is a tendency in the universe to change the superior kinds of energy into inferior or degraded kinds, which latter can only to a very small extent be changed back again into superior forms. Thus we have seen how easy it is by percussion or friction to transmute all the mechanical energy of a blow or visible motion into heat, but only a very small portion of this heat can be transmuted back into visible motion. There is, in fact, a tendency abroad to change all kinds of energy into low-temperature heat equally spread about,—a thing that is of no possible use to anyone.

Seeing, then, that our existence and well-being depend on the presence in the universe of a large quantity of superior energy, which we may be able to utilize, it becomes us to look about us, and take stock as it were of the goods that have been placed at our disposal. Now the nearest approach to an ever-burning lamp is the sun, and a near approach to a perpetual motion is represented by the motion of the earth on its axis, and it will shortly appear that it is from these two sources of superior energy that we draw all our supplies of this indispensable commodity.

Importance of sun.

Of the two sources the sun is by far the most important. Let us examine very briefly the extent of our obligations to our great luminary. In the first place, without his energy in the shape of heat and light everything in the world would be frozen and dark; for the little heat left, being unrecruited, would very soon pass off into space, and our scanty stock of fuel would form a very poor substitute for the sun's rays. But this is only a small part of what we get from the sun, for we have already hinted that it is by means of the energy of his rays as absorbed by the leaves of plants that carbonic acid is decomposed, and coal and wood produced, coal being a product of the past and wood of the present age.

Food=fuel.

Food has the same origin as fuel; it is in fact the fuel which we burn in our own bodies instead of on our hearths or in our engines. Without a proper supply of food we should soon cease firstly to perform work and ultimately to live, and the more hard work we have to accomplish the more food must be taken.

In like manner, without a proper supply of fuel a steam-engine would soon cease to perform work. Again, wind and water power, or the power of air and water in motion, ought not to be forgotten as forms of energy which may be usefully applied. These also are indirectly due to our luminary, whose heat produces currents in the atmosphere, and also carries up in the form of vapour the waters of the ocean to be again precipitated in the form of rain. Windmills and watermills are therefore due to the sun as well as steam-power and muscular energy. Tidal energy stands, however, on another footing. The tides are produced by the action of the moon and of the sun upon the waters of the ocean, but the energy which they represent is not derived from these luminaries, but from the rotative energy of our own globe, which is gradually losing its speed of rotation from this cause, although at a rate which is extremely small, indeed almost infinitesimal.

Is it then the case that we have been furnished on a grand scale with that which enthusiasts have in vain tried to imitate on a small one, namely,—an ever-burning light and a perpetual motion?

If we allow that myriads of years bear a nearer approach to eternity than a few hours, then we may assert that this is the case; but if we regard all duration and all magnitude as comparative, then we have only been furnished on a large scale as regards both these elements with what we can ourselves produce on a small one.

The principle of degradation is at work throughout the universe, not less surely, but only more slowly, than when it combats our puny efforts, and it will ultimately render, it may be, the whole universe, but more assuredly that portion of it with which we are connected, unfit for the habitation of beings like ourselves. As far as we are able to judge, the life of the universe will come to an end not less certainly, but only more slowly, than the life of him who pens these lines or of those who read them.

It is desirable to state clearly, and once for all, that our standpoint in what follows is that of students of physical science. We are here only as such students, and, from the trifling elevation which we may have reached as followers of science, we shall endeavour to answer, it may be imperfectly, but yet honestly, certain questions which might be put to us by those who are interested in knowing "how the day goes."

More particularly then with regard to the place of life, —What are the conditions necessary in order that the universe may be a fit abode for living beings?

Delicacy of organization.

It has already been shown that one of these conditions is the existence in the universe of a quantity of energy, not in a thoroughly degraded state, but capable of producing useful effect; we have now to add that *another condition is the capability of great delicacy of organization.*

The motion of the universe would seem to be of two kinds; it is in fact the old story of a shield with two sides, each side with its champion, and the quarrel between them very hot. If we reflect, we shall see that the perfection of the laws which regulate the larger masses of the universe, such as planets, consists in the fact that the motions produced are eminently capable of being made the subject of calculation. But, on the other hand, the very perfection of the animated beings of the universe consists in the fact that their motions cannot possibly be made the subject of calculation. A man who could predict his own motion is an inconceivable monster; in fact, having calculated what he is about to do, he has only to do the opposite in order to show the absurdity of the hypothesis.

This freedom which is given to animated beings is nevertheless held quite in conformity with, and in subjection to, the laws of energy already mentioned, but it requires as a condition of its existence *great delicacy of organization.*

In order to comprehend what is meant by this expression, we may imagine to ourselves a universe consisting of nothing but carbon and oxygen separate from one another. Such a universe would possess to a very large extent a superior kind of energy, yet we cannot by any possibility imagine how such materials could be moulded into organized forms or become the residence of living beings. The very idea of its sable monsters provokes a smile, although we might perhaps be at a loss were we asked definitely to state our objection to this condition of things.

Let us, however, consider this imaginary universe for a moment, and the nature of its deficiency will soon appear. If on fire, it will continue to burn at a rate which may be calculated without much trouble; if not on fire, it will continue as it is. There is not, therefore, in such a universe any, or hardly any, capacity for producing or sustaining delicate organizations possessing freedom of motion.

A living being (at least one of a superior order) is not only a machine capable of producing motion, but of producing it discontinuously, and in a great variety of ways which cannot be calculated upon except to a very limited extent.

In this respect there is a class of machines analogous to some extent to living bodies. Suppose, for instance, a gun loaded with powder and ball, and very delicately poised; then by the expenditure of a very small amount of energy upon the trigger, a stupendous mechanical result may be achieved, which may be greatly varied: touch the trigger, and the gun is discharged, driving out the ball with great velocity. The direction of its path will, however, depend upon the pointing of the gun; if well pointed, it may explode a magazine,—nay, even win an empire.

Here, then, there is a very stupendous result in the way of visible motion produced through the agency of a very small amount of energy bestowed upon the trigger, and all in conformity with the conservation of energy, since it is a certain kind of energy of position resident in the gunpowder that has been changed into mechanical effect; but yet the result cannot be achieved without the application of this small amount of directive energy to the trigger, for if the trigger be touched too lightly the gun will not go off. The small amount of energy bestowed upon the trigger becomes, as it were, the parent or source of the much larger amount of energy of the cannon-ball. We have in fact here a machine of *great* though *finite* delicacy of construction.

It is not, however, impossible to suppose a machine of *infinite delicacy of construction.* We may, for instance, imagine an electric arrangement so delicate that by an amount of directive energy less than any assignable quantity a current may be made to start suddenly, cross the Atlantic, and (as far as physical results are concerned) explode a magazine on the other side. Indeed, the forces of nature appear to be such that an infinite delicacy of construction is not inconceivable.

We have thus considered two cases of machines having great delicacy of construction. In the former of these it required a certain finite and definite amount of energy to be expended on the trigger before the gun was discharged, but in the second case things were brought to such a pass that by an application of an amount of energy less than any assignable quantity, the electric circuit would be rendered complete. The first case in fact represents a machine of great but yet finite delicacy; the latter, a machine of infinite delicacy of construction.

Functions of life.

Let us now proceed to state the various conceivable functions that life may be supposed to discharge with relation to the energy of the universe: we say conceivable, for in the sequel the reader will be called on to select from a list of four kinds of action, of which two, although conceivable, are yet extremely improbable. Our choice therefore must finally be restricted to two conceptions, neither of which is inconceivable or impossible as far as the laws of energy are concerned; and between these two we must finally choose on other grounds than can with propriety be treated of in this article.

There are four functions which life or intelligence may be supposed to discharge. In the first place, there is the purely materialistic view of life, which may be stated thus:—

Materialistic view.

A living being is a very complicated machine, consisting of matter very delicately organized, but containing besides no other principle; so that, if we knew completely the laws of matter and the position of the various particles which constitute the machine, and if we knew at the same moment the disposition of the exterior universe which is capable of influencing the machine, and if our methods of calculation were sufficiently developed, we should be able to predict all future motions of the living being.

Second hypothesis.

The second hypothesis is, that life or intelligence has the capacity for creating energy. This view is so very improbable that we may dismiss it with a very few remarks. What we can say with truth is that, in all experiments and observations which we have been able to examine thoroughly, energy is not created. It is conceivable that there may be a region beyond our ken in which energy is created, but, arguing according to the principles which are universally admitted to be our guides in such matters, we must pronounce the creation of energy by a living being to be out of the question.[1]

Third hypothesis.

The third conceivable hypothesis regarding the function of life is that which asserts that life, although it cannot create energy, can yet transmute *immediately, and by virtue of its presence*, a finite quantity of energy from one form to another. It is necessary to explain the meaning of the word *immediately*. Referring to the gun with a delicate trigger, which we have already alluded to, it cannot be said that the *immediate* cause of the motion of the ball was the energy bestowed upon the trigger: the immediate cause of this motion was the aëriform state which the gunpowder had assumed, while again the immediate cause of the change of state in the gunpowder was the heat developed by the explosion of the fulminating powder in the touch-hole, and the cause of the powder's exploding was the blow given to it by the hammer of the lock. The blow again may be traced to the action of the lock-spring, which is set free to act through the small impulse communicated to the trigger. We see from this, that whenever a finite amount of energy changes its form,—as, for instance, when the chemical energy of the gunpowder is changed into the mechanical energy of the ball,—we naturally look to some material circumstance which precedes and explains this change. We may be quite certain that the gunpowder will not explode unless a small quantity of high-temperature heat be communicated to it, nor will the fulminating powder explode unless it receives the blow, nor will the blow be given unless the trigger is pulled.

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[1] This was recognized at an early period by Carpenter and Joule.

Thus, in this example, if we are able to change some energy which we have at hand into visible energy sufficient to pull the trigger, that small change will form the original germ of the much greater one implied in the explosion of the powder and the motion of the ball, or rather it will be the first link in a series of changes of which the last is the motion of the ball; and so in similar machines we find a change of energy preceded by some other change, perhaps much smaller in amount, which explains it. And now the question arises, Can life, while it does not create energy, be yet the *immediate* cause of the change of a finite quantity of energy from one form to another, which change would not have taken place without the presence of life, and which is not, therefore, preceded by a material cause in the shape of a parent change of energy? We cannot readily allow that life can act thus, for this would imply that of the finite and measurable changes of energy which take place in the universe, and which therefore either are, or may become, subjects of experiment and observation, some are immediately preceded by a material cause, and some by an immaterial one, and that this is the regular system of things: to the minds of most men an uncertainty of this nature in the immediate causes of measurable results will appear improbable *à priori*, and, moreover, it is a view entirely unsupported by experiment and observation. Let us, therefore, dismiss this view of the action of life, and consider the only other view of its action which appears to be possible.

Fourth hypothesis.

Assuming, therefore, that life can neither create energy nor yet immediately transform a finite amount of energy from one form to another, may not the living being be an organization of infinite delicacy, by means of which a principle in its essence distinct from matter, by impressing upon it an infinitely small amount of directive energy, may bring about perceptible results? We have shown that such a class of machine is conceivable, when we suggested a certain electrical arrangement, and we know that our bodies are machines of exquisite delicacy. Such a mode of action of the vital principle is not, therefore, inconceivable, and, by supposing that it does not immediately change a finite quantity of energy from one form to another, we get rid of that element of irregularity which we cannot easily admit to be consistent with the order of nature.

We come to two hypotheses.

We are thus presented with two hypotheses of the action of life. The first of these is the materialistic hypothesis, which denies the existence of life as a principle apart from matter; while the other allows the existence of an independent principle, but assumes its action to take place through the medium of a machine of infinite delicacy, so that by a primordial impulse of less than any assignable amount a finite and visible outcome is produced. These are the two alternatives, and it is not within our province to attempt to decide between them. The battle must be fought in other pages than ours, and by other weapons than those which we can produce.

Principle of delicacy.

Let us here pause for a moment to consider the wonderful principle of delicacy which appears to pervade the universe of life. We see how from an exceedingly small primordial impulse great and visible results are produced. In the mysterious brain chamber of the solitary student we conceive some obscure transmutation of energy. Light is, however, thrown upon one of the laws of nature; the transcendent power of steam as a motive agent has, let us imagine, been grasped by the human mind. Presently the scene widens, and, as we proceed, a solitary engine is seen to be performing, and in a laborious way converting heat into work; we proceed further and further until the prospect expands into a scene of glorious triumph, and the imperceptible streamlet of thought that rose so obscurely has swelled into a mighty river, on which all the projects of humanity are embarked.

And now a hint to those who are disposed to adopt that theory of life which demands an infinite delicacy of construction.

Is there action at a distance?

May it not be possible that in certain states of excitement there is action at a distance? This is a field of inquiry which men of science do not seem disposed to enter, and the consequence is that it appears to be given over to impostors. We need scarcely, after this, inform the reader that we do not believe in so-called spiritual manifestations; nevertheless we ask, does there not appear to be an amount of floating evidence for impressions derived from a distance in a way that we cannot explain? For are not the most curious and inexplicable actions of instinct those in which distance seems to be set at nought? Then, again, if we take the element time, instead of distance:—who has not felt some past scenes, perhaps of his early childhood, called up suddenly and vividly before him by some trivial sight, or sound, or smell? May there not, after all, be a deep physical meaning in these words of the poet:—

> "Yet still, from time to time, vague and forlorn,
> From the soul's subterranean depth upborne,
> As from an infinitely distant land
> Come airs and floating echoes, and convey
> A melancholy unto all our day."

Application to the sun.

Hitherto we have been confining our thoughts to the realms of life, in which the principle of delicacy is sufficiently obvious, but the results of a preceding article will have prepared our readers for a wider application of this principle. It is not only in the organic world that we see a delicacy of construction, but in the inorganic also. Thus it will be remembered that, in discussing the molecular state of the sun, we came to the conclusion that it was one of great delicacy, so that in our luminary a very small cause might be the parent of enormous effects, of a visible and mechanical nature. And when we came to analyse the behaviour of sun-spots, we found that this behaviour had a manifest relation to the positions of the two planets Venus and Jupiter, although these two planets are never so near the sun as they are to our own Earth. We have also shown that sun-spots or solar disturbances appear to be accompanied by disturbances of the earth's magnetism, and these again by auroral displays. Besides this, we have some reason to suppose a connection between sun-spots and the meteorology of our globe. From all these circumstances we cannot fail to remark that the different members of our system (and the thought may be extended to other systems) are more closely bound together than has been hitherto supposed. Mutual relations of a mathematical nature we were aware of before, but the connection seems to be much more intimate than this—they feel, they throb together, they are pervaded by a principle of delicacy even as we are ourselves.

We remark, in conclusion, that something of this kind might be expected if we suppose that a Supreme Intelligence, without interfering with the ordinary laws of matter, pervades the universe, exercising a directive energy capable of comparison with that which is exercised by a living being. In both cases delicacy of construction would appear to be the thing required for an action of this nature.

Bearing in mind, however, our physical standpoint, we cannot venture to offer any further remark on this subject. Whether such a mode of action is a *fact* must be decided by other considerations; whether it would appear to be *physically possible* is a question which we may suppose put to us, and which we have ventured to answer as above.

THE PLACE IN SCIENCE OF THE NEW METHOD.

CHAP. VIII. THE year that has just passed from us[1] will be for ever memorable in the history of science. It has given to astronomers a method of studying the physics of our central luminary, the sun, at once so delicate and so searching that a new era of observation has dawned; it has, moreover, seen the settlement of a question which has puzzled mankind for a century and a half, and put us in possession of new facts of the highest interest and importance.

The sun shining in unclouded and uneclipsed splendour is a sight familiar to us all, and, time out of mind, mankind has watched his risings and settings, and poets have sung of the panoply of cloud and glory in which he then appears. In these ordinary aspects, however, the sun for the last 150 years has been invested with comparatively little interest to the astronomer; it is only when hid in dire eclipse that the feeble human eye can appreciate all the wonders of our great light-giver—and total eclipses happen but very rarely.

Whenever the sun shines, the brilliant envelope, called the photosphere, is full of wondrous teachings; and the marvellous sun-spots, the discovery of which sent a shock as of an earthquake through the minds of the Schoolmen at the beginning of the seventeenth century, are among the most striking and stupendous effects of force which it is given to man to witness. But these are *now* but ordinary phenomena; we are familiar with them; and we are apt to forget the scale on which the changes rendered visible to us by our telescopes take place. This is not the case with the different classes of actions which, though for ever going on, are only visible to us when, during eclipses, the moon shields our eye, and, by interposing herself exactly between us and the sun, allows us to inspect the sun's atmosphere with perfect ease. Then new glories are rendered visible, which make the moments of the totality as precious to scientific men as they are terrible and awe-inspiring to ordinary beholders. One seems in a new world—a world filled with awful sights and strange forebodings, and in which stillness and sadness reign supreme; the voice of man and the cries of animals are hushed; the clouds are full of threatenings and put on unearthly hues: dusky livid, or purple, or yellowish crimson tones chase each other over the sky irrespective of the clouds. The very sea is responsive, and turns lurid red. All at once the moon's shadow comes sweeping over air and earth and sky with frightful speed. Men look at each other and behold, as it were, corpses, and the sun's light is lost.

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[1] This was written in 1868.

Phenomena of totality.

The work of the astronomer.

And then? Then the astronomer at his telescope sees the edge of the moon, which hangs like a black ball in the heavens, suddenly, as if at the call of a magician, bedeckt here and there with strange-looking tongues of red flame, and at those parts where the edges of the sun and moon most nearly fit each other lower ridges are seen, lying close to the moon's edge—or limb—and continuous for some distance. As the moon travels over the sun from west to east, these strange "red flames," still maintaining their shapes, vary their size, and then, just before totality is over, another line of ridges is seen on the opposite part of the moon to that on which they first appeared. Then the sun appears again, and these strange things are lost to human eye till another total eclipse comes round with its glorious revelations.

Red flames.

The discoveries to which we have now to draw attention refer to the nature of the "red flames," "prominences," or "protuberances," as they have been variously called, visible, as we have seen, during total eclipses of the sun; but to discuss these discoveries at all usefully it is necessary that we should trace up our knowledge of the red flames themselves, and state the optical principles on which the discoveries are based.

Growth of our knowledge.

First, then, as to the growth of our knowledge concerning the red flames. As far as I know, the first mention of those strangely beautiful and weird appendages of the sun, variously called "red flames," "prominences," and "protuberances," which are visible, and only visible, during a total eclipse of our great luminary, occurs in a letter addressed by a Captain Stannyan to Flamsteed, in 1706; that is, 162 years ago. Stannyan was at Berne, observing the total solar eclipse of that year, when the sun was totally darkened for four minutes and a half; he seems to have had sufficient presence of mind to have given the marvellous and awful accompanying phenomena only their due share of attention; for he carefully watched for the sun's reappearance, and was rewarded by observing that "his getting out of his eclipse was preceded by a blood-red streak of light from his left limb, which continued not longer than six or seven seconds of time; then part of the sun's disc appeared all of a sudden as bright as Venus was ever seen in the night—nay brighter; and in that very instant gave a light and shadow to things as strong as the moon uses to do." It seems pretty clear that Stannyan believed this "blood-red streak" to belong to the sun, for he does not mention the moon; but unfortunately authority, in the shape of Flamsteed, referred it without question to the moon; and the height of our satellite's atmosphere was at once calculated to a nicety.

Stannyan.

Halley and Louville.

A similar phenomenon was recorded by Halley and Louville in 1715—the former describing "a long and very narrow streak of a dusky but strong red light," which "seemed to colour the dark edge of the moon;" the latter seeing "an arc of a deep red colour." These, doubtless, were the equivalents of the "streak" seen by Stannyan, and were not "prominences" properly so called. The theory that these strange things belonged to the moon and not to the sun was not, however, shaken by these observations; and when Vassenius described to the Royal Society the eclipse of 1733, he boldly placed the "reddish spots" which he observed "in the lunar atmosphere." It may be safely stated that until the year 1842 the idea that the red flames were entities in, or appearances caused by, the moon's atmosphere, was never called in question; and it was not banished from men's minds till the year 1860. It held its own, therefore, for over a century and a half—a pretty long run for an assertion made on such slender basis, but one not altogether unprecedented.

Vassenius.

From 1706 to 1860, total eclipses of the sun have swept over Europe. I believe that in every case—certainly in every late case—the remarkable phenomena first observed in 1706 have been seen. In astronomical observation as in other matters, *Ce n'est que le premier pas qui coûte* —the mind helps the eye as well as the eye the mind; but the records are singularly unsatisfactory till the year 1842 is reached, and then the golden age begins. The "red flames" in that year were watched by several observers of the highest eminence. It is the first eclipse, in fact, of which we have full and scientifically accurate observations; as a consequence, our knowledge of the rose-coloured prominences was largely increased. They were measured with the utmost care, and their various colours and general appearances were recorded. Mauvais compared two of the prominences in shape and colour to the peaks of the Alps illuminated by the setting sun. Mr. Airy likened them to saw-teeth in the position proper for a

Eclipse of 1842.

circular saw. Mr. Baily describes those seen by himself encircling the black moon as follows:—They had the appearance of mountains of a prodigious elevation; their colour was red, tinged with lilac or purple; perhaps the colour of the peach-blossom would more nearly represent it. They somewhat resembled the snowy tops of the "Alpine mountains when coloured by the rising or setting sun." One observer poetically remarked that if the luminous points he saw had extended all round the moon it would have resembled "a box of ebony garnished with rubies." Littrow saw them change from white to red and from red to violet, and then back again through the reverse order.

Mauvais and Petit's observations.

But the most important observations made during this eclipse have yet to be referred to. Mauvais, a French astronomer who was stationed at Perpignan, saw a reddish point transform itself into two large protuberances, and soon a third began to be visible to the left of them. While the third "mountain" appeared to be issuing forth, the first two observed continued to increase. Further, Petit, another French astronomer, also saw them rapidly increase in magnitude as if they were uncovered in consequence of the motion of the moon—in fact, they seemed to emerge from the eclipse *pari passu* with the sun. Here then, apparently, was evidence that they belonged after all to the sun, and not to the moon.

Eclipse of 1851.

The next swing of the eclipse pendulum brings us to 1851, and to Sweden, which was in consequence the rendezvous of European, but especially of English, astronomers, who were now convinced, particularly by the observations of 1842, of the enormous interest and importance of the problem. Of this eclipse we have admirable records. Airy, Adams, Dawes, Hind, Carrington, Robinson, Dunkin, Lassell, were among the eminent observers who were there to endeavour to settle the question. Prominences there were in abundance, some of them of great magnitude and striking form. So enormous in height and so brilliant was one of them, that it was clearly visible to the naked eye, and Dawes saw it five seconds after the sun had reappeared! We owe to that lamented observer the most minute account of the prominences. One of them was cone-shaped, of a deep red colour; another was a bluntly triangular pink body, disconnected with the sun; and another like a "Turkish cimeter," 70,000 miles in height if it belonged to the sun, with one of its edges of a rich carmine colour. Besides these, two low ridges were seen stretching along the moon's edge; in one of them was a flame like a "dog's tusk," its colour and brilliancy varying from those of the lower ridges. The prominence which reminded Dawes of a "cimeter" was likened by Airy to a "boomerang," its colour "full lake red;" the latter also saw one of the ridges, which he called a "sierra," situated along the sun's edge at the part where it was just fitted by the edge of the moon; this sierra being more brilliant than the other prominences, and its colour scarlet. This eclipse left a very distinct impression on the Astronomer Royal's mind as to the exact place of the prominences. It was impossible, he said, to see the changes that took place in the prominences without feeling the conviction that they belonged to the "sun and not to the moon." Still Professor Adams was not quite convinced. Mr. Dunkin held a contrary opinion to Airy; other observers, if they had formed one, did not express it; and we believe that the general *consensus* will be faithfully represented by saying that at this period, the whereabouts of the prominences—*i.e.* whether they belonged to the sun or moon—was "not proven." There was strong evidence going to show that they could only belong to the sun, but the theory was not thoroughly established.

Verdict, "not proven."

Still the eclipse of 1851 was not without its results. The prominences lent themselves admirably to minute observations, the work done in former years was endorsed, and the old mountain-shaped prominences and the low sierras, of a different colour from the prominences proper, and more brilliant, were again observed.[1]

Eclipse of 1860.

The next attack was made in 1860, the astronomical forces having in the meantime secured an ally of tremendous power. By this time celestial photography, in the hands of Mr. Warren De la Rue, had arrived at a high state of perfection. And now *the prominences were photo-*

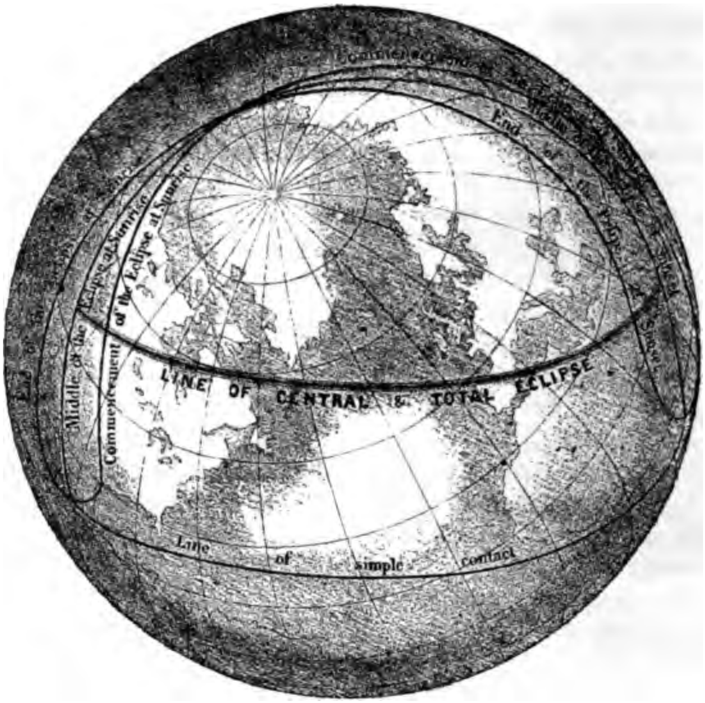
Commencement of the Eclipse at Sunrise
Middle of the Eclipse at Sunrise
End of the
End of the Eclipse
Sunset
LINE OF CENTRAL & TOTAL ECLIPSE
Line of simple contact

FIG. 22.—Path of Moon's shadow during Eclipse of 1860.

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[1] Since the articles were written I have learnt that the name of Mr. Swan should have been added, for among the most important results of this eclipse are three papers communicated by him to the Royal Society of Edinburgh, in which he clearly points out, as Grant had done for the observations before 1851, that the prominences are part of the sun, *and that there is a continuous stratum of prominence matter surrounding the sun.*

graphed, not only by Mr. De la Rue himself, but by Father Secchi, who has followed in the wake of all work of this nature. The prominences now told their own story on his photographic plates, and announced themselves as belonging beyond all question to the sun. It is impossible to speak too highly of the skill with which the aid of this new method of recording astronomical phenomena was invoked and utilized by Mr. De la Rue, in spite of the probability that his work would all be useless. He writes:—"The general impression I formed from the information derived was that the light emitted by the corona" (a halo of dim white light which surrounds the moon during total eclipses) "and red flames, taken together, was about equal to that of a full moon—less rather than greater; but no one recollected precisely the brightness of the prominences as compared with that of the corona. With this imperfect information as a guide, an attempt was made at Kew to photograph the moon, but not the slightest impression could be procured of our satellite by an exposure of the sensitive plate during one whole minute to its image in the heliograph. My expectation of success in getting pictures of the totality was not great after this trial; nevertheless, I still thought it desirable to carry on the experiment to the end, on account of the value of the results, if I should fortunately succeed."

Photographic results.

Mr. De la Rue was able to obtain the sun's own evidence of the famous Spanish eclipse in an almost unbroken series of upwards of forty photographs, from the time the moon made her first appearance on the sun till the time she had entirely crossed it. Just before the sun was totally hid, the prominences became visible in the telescope, and were recorded on the photographic plate; a long line of low ridges being visible when the eastern edge of the moon, which was travelling from west to east, was coincident with the just hidden edge of the sun. Tops of high prominences were also registered where the moon (which appeared much larger than the sun) extended grossly beyond the sun's edge, especially

Comparison of photographs.

careful comparison of them with those taken by Father Secchi, who observed the eclipse at some distance from his station; and he found important differences in them—exactly such differences, too, as must have arisen from the difference of position of the observers if the prominences really belonged to the sun. To thoroughly understand this, let the reader slide a shilling representing the moon over a sixpence representing the sun, from right to left, *i.e.* west to east.

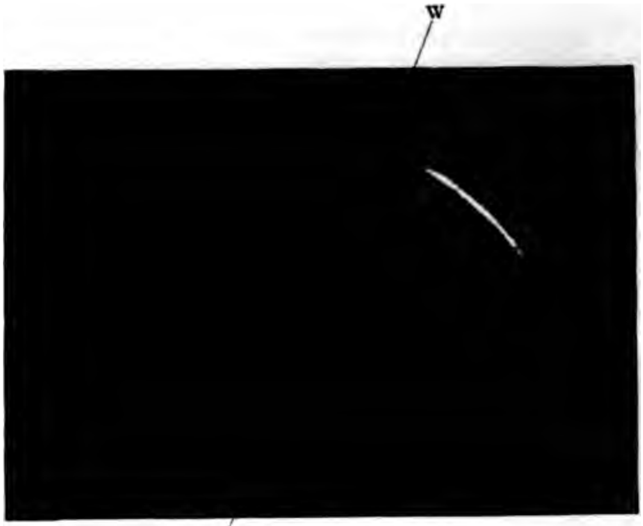

FIG 25—Total Eclipse (Indian), Aug. 18, 1868: end of totality. The prominences shown in Fig. 24 are now still more uncovered.

Results

It was distinctly evident that the elevation of the prominences above the moon's northern limb was much higher in Mr. De la Rue's pictures than in Father Secchi's; a fact accounted for by the moon having been seen much higher at Desierta (Father Secchi's station) than at Rivabellosa, where Mr. De la Rue was. Similarly, the prominences seen beyond the moon's southern limb were most uncovered in Secchi's photographs.

Question as to whereabouts of prominences settled.

In the year 1860, then, was settled the question as to the position of the strange things first observed in 1706, and the settlement of this point had brought with it large additions to our knowledge concerning their shapes, sizes, colours, and the like.

One of the prominences registered by Mr. De la Rue, for instance, extended 72,000 miles from the sun's surface into its atmosphere; and there was also evidence that the substance of which some of them were composed *gave out light much stronger chemically than visually, for they allowed themselves to be registered on the photographic plate while they were invisible to the eye.* These and innumerable other points of interest, to which we cannot do more than refer, lent enormous importance and emphasis to the next question that was asked of these strange things.

What are the prominences?

"What are they?" was on the lips of all interested in science. But how to tell what they were? How to solve this final riddle? Telescope, camera, and eye had done their utmost, but their all was unavailing in such an inquiry as this. The astronomers were baffled; they not only knew they were ignorant, but they saw no way out of their difficulties—there was no other line of research open to them.

Spectrum analysis.

In science, however, as in other things, help sometimes comes from a very unexpected quarter. It was so in this case. Before even the *Himalaya* had started with its freight of astronomers to view the famous Spanish eclipse, two men had been working independently, one in Heidelberg and the other in Edinburgh, on the subject of radiant light and heat, and the final result of the labours of these physicists, Kirchhoff and Balfour Stewart, was that at last the world of science was put in possession of just such a method of research as we have shown it coveted in order to settle the nature of the red flames.

The researches of Kirchhoff showed, in fact, that the light proceeding from any substance contains, as it were, an autobiography of that substance—in a strange language, certainly, but one capable of being translated into the vulgar tongue by passing a light through a prism. If, for instance, we look at the flame of a candle through a common lustre, we find the yellow—almost white—light which it gives out transformed into a broad, rainbow-coloured band; a continuous riband of coloured light, red at one end and passing through yellow, green, blue, and indigo to lavender at the other. If, with certain precautions which need not be stated here, we examine the light given out by a gas, or by the vapour of burning substances, such as sodium, iron, &c., we find this continuously lighted-up riband replaced by bright lines, scattered here and there along the space that would be covered by the band of coloured light if we were looking at a candle. In the fact that these bright lines vary with every substance which we can examine we have the basis of *Spectrum Analysis*.

Connection with the sun.

But so far we have said nothing of the connection of this discovery with the sun. The sun's spectrum is not like either of those previously referred to. The light is not continuous, and it does not consist of bright lines. It is coloured exactly as it would be if it were continuous, like the spectrum of a candle, but crossing it at right angles to its length, are innumerable black lines or bars of greater or less thickness. Kirchhoff—and this is the great merit of his discovery—explained these lines. He pointed out, first of all, that many of them occupied exactly the same position in the spectrum as the bright lines given out by such substances as sodium, iron, &c., do; and he next showed that it was possible to reproduce such a spectrum at will, by allowing a bright light to shine through the vapour of sodium, iron, &c., in which case the cool vapour uses up the light of the same colour that it itself would emit, and the otherwise continuous spectrum of the bright light reaches the eye with a bar across it. We may sum up Kirchhoff's results as follows:—

The main points of spectrum analysis.

1. When solid or liquid bodies are in a state of incandescence they give out continuous spectra.

2. When solid or liquid bodies, reduced to a state of gas, or any gas itself burns at ordinary pressure, the spectrum given out consists of bright lines only, *and these bright lines are different for different substances.*

3. When light from a solid or liquid incandescent body passes through a gas, the gas absorbs those particular rays of light of which its own spectrum consists.

Elements in sun.

By carefully mapping the lines in the solar spectrum, and by matching them with the bright lines given out by gases and the vapours of solid substances, Kirchhoff established beyond all question that sodium, iron, magnesium, barium, copper, zinc, calcium, chromium, nickel, and aluminium were present in the sun's atmosphere: to these hydrogen has since been added.

The reader should now be able to see the drift of what we have stated with regard to the application of this method of research to the red flames. An analysis of their light must at once give us information as to their solid or gaseous nature. If the light they emitted gave a continuous spectrum, then we should say they were either solid or liquid. If, on the contrary, their spectrum consisted of bright lines, then we should know that they were gaseous. In the latter case the particular gas or vapour would be determined by the position of the bright lines in the spectrum.

New attack.

This problem was attacked in 1866, and without waiting for an eclipse!

I had been carefully investigating sun-spots both with telescope and spectroscope (an instrument which consists of carefully-arranged prisms), and had succeeded in showing by means of the latter marvellous means of research that they were due to a greater absorption of the solar atmosphere in certain localities; and having had the benefit of several conversations with my eminent friend Dr. Balfour Stewart, the conclusion we arrived at was that the red flames were probably masses of incandescent gas.

Failure.

Reasons for failure.

On this hypothesis it became at once obvious that their existence should be revealed by the spectroscope without the occurrence of a total eclipse, as they are not then rendered visible by any magical or mysterious process, but simply by the absence of the overpowering light of the sun: for although the red flames are only visible *to the eye* when the sun is eclipsed, it does not follow that their existence will not be detected *by the spectroscope* at other times; and for this reason,—the prominences are not visible to the eye in ordinary sunshine, because the regions near the sun are as bright or brighter than the prominences; they are, therefore, "put out," as the stars are in daytime. But mark what will happen if they really be built up of gas, and their light in the spectrum is concentrated into a few bright bands. The light of the sun's atmosphere, which is made to enter the instrument through an excessively narrow slit, will be spread out into a long band; the light will cover a large area and will become *diluted* in consequence. But the light from the prominences (coming through the same slit) will, on the contrary, scarcely be spread out at all, it will remain concentrated in two or three or more lines, and in the spectroscope the prominence lines should for this reason be seen as *bright* lines on the *fainter* background of the spectrum of the atmosphere. I began to act upon this idea in 1866; but the only result of my efforts was to show me that the means at my disposal were not sufficient to attack the problem with any chance of success. It was essential that I should obtain the spectrum of the edge of the sun and the regions just outside it, and that the latter should be dark enough to form a background for the bright lines that would be seen here and there projecting from the solar spectrum if the hypothesis that the prominences were gaseous was correct. In my instrument, however, the illumination of the sun's atmosphere by the light reflected from the outer bright shell of the sun itself —the photosphere—and the illumination of our atmosphere especially were so great near the sun, that the background was not dark enough to allow bright lines to be easily visible, and I failed to detect any lines, though I diligently "fished" round the sun's limb many times, and in all probability passed over prominences. I therefore communicated my idea to the Royal Society, and my difficulties to the Government Grant Committee. The matter was thought worthy of their aid, and in the beginning of 1867 an instrument was being constructed, which owing to a chapter of accidents I only received incomplete on the 16th of October, 1868. Owing to these delays in the construction of this instrument, the solution of the question was left to the eclipse of 1868, visible in India.

Indian eclipse, 1868.

Nearly all the principal scientific bodies of Europe took up the matter with great warmth. Our own Royal Society and the Royal Astronomical Society both sent out expeditions, and so did the Academy of Sciences and the Bureau des Longitudes. Prussia and Austria were also represented. The eclipse was to happen on the 18th of August, and the results were anticipated with the utmost impatience and interest. The result of the observations in India was decisive as to the nature of the prominences. The spectroscope settled this as satisfactorily as the camera had settled their whereabouts in 1860. At last the telegrams came. The two words "bright lines," were quite sufficient to tell the scientific world that one large part of the problem had been settled. The "red flames" were really built up of glowing gas or vapour. All the observers had seen those tell-tale lines during the eclipse which had in vain been looked for in the full glory of the sun with my small instrument. One large part of the final question was for ever put to rest. The prominences were built up of incandescent gas or vapour.

"Bright lines."

One question still unsettled

But what gas, or what vapour? This would be indicated by the relative positions of the bright lines referred to the solar spectrum itself—that glorious band of rainbow hue, from red through orange, yellow, green, blue, indigo, and lavender, crossed at right angles to its length by innumerable black lines, which is the very cypher of the universe, but which nevertheless has been read a little. With which of the black lines did these newly-discovered and

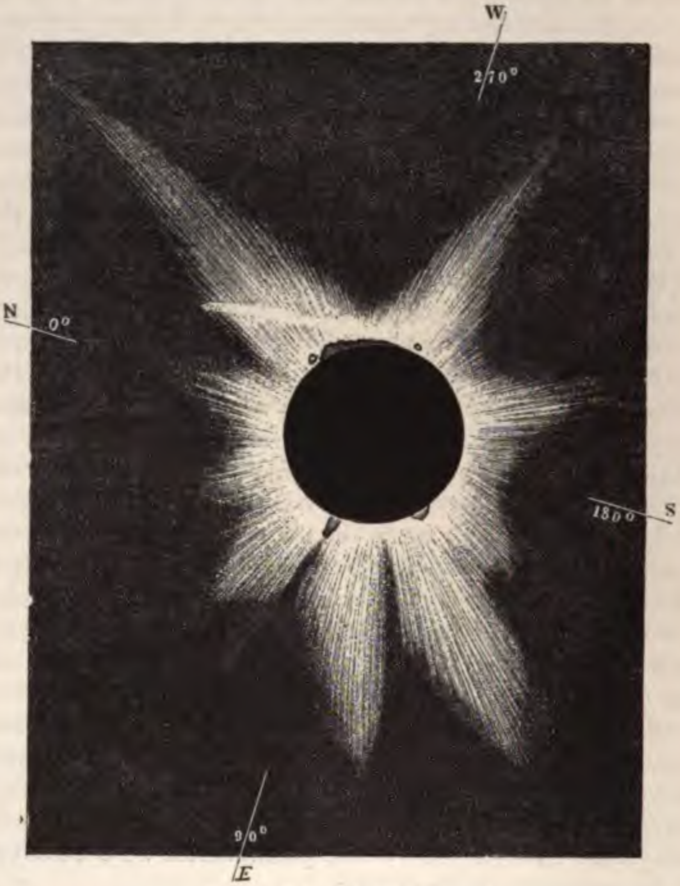

Fig. 26.—Eclipse of 1868. (Corona.)

all-eloquent bright lines coincide? Here the eclipse gave out an uncertain sound. In due course of time we received detailed accounts from three of the expeditions, under Captain Herschel, Major Tennant, and M. Stephan respectively, and representing the Royal Society, the Royal Astronomical Society, and the Bureau des Longitudes. In the detailed accounts, although it was clear that every man had tried his utmost, there was not such great subject of congratulation as there had been in the telegrams. There was still one part of the question unsettled. All the observers had observed bright lines, but they were not certain as to the positions of some of the lines, and the accounts were discordant among themselves. Premising that the principal black lines in the solar spectrum are lettered from the red towards the lavender end, the following results will be readily understood. Captain Herschel wrote:—

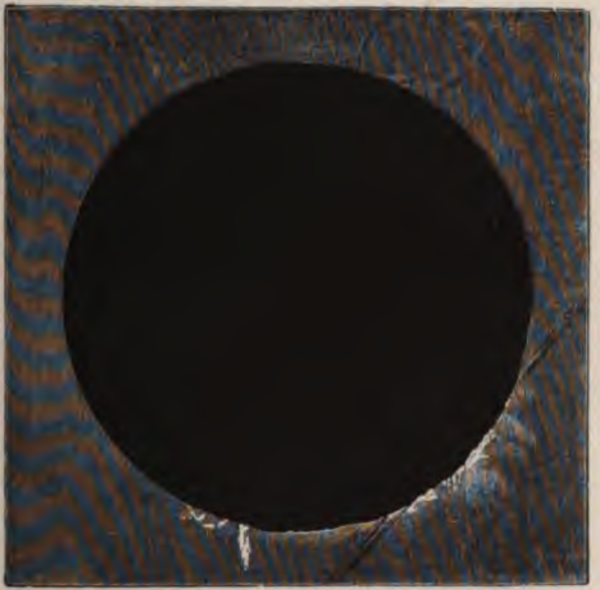

FIG. 27.—Copy of Tennant's photograph: beginning of totality.

Herschel.

"I consider that there can be no question that the orange line was identical with D, so far as the instrument is competent to establish an identity. I also consider that the identity of the blue line with F is not established, but, on the contrary, I believe the former is less refracted than F, but not much. With regard to the red line, I hesitate very much in assigning an approximate place. It might have been near C; I doubt its being as far as B, but there would be the limit. I am not prepared to hazard any more definite opinion about it. Its colour was a bright red."

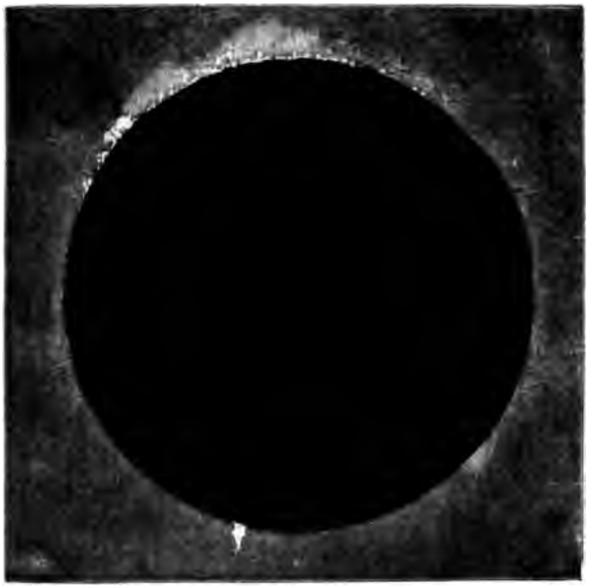

FIG. 28.—Copy of Tennant's photograph: end of totality.

Tennant.

Major Tennant saw five lines, three of them corresponding to C, D, and *b*, one in the green near F, and possibly one near G.

Rayet.

M. Rayet (in M. Stephan's party) saw nine lines, corresponding to B, D, E, *b*, F, and the others undetermined.

Discrepancies.

It is obvious from these discrepancies, which we hold to be entirely unavoidable in such a delicate investigation, carried on under difficulties of weather and under conditions so out of the common, that the question as to the nature of the red flames—our knowledge of which depended upon a rigorous determination of the position of the lines—was left open; and it seemed very much as if, after all, the problem would be left in uncertainty and doubt until another total eclipse with its attendant phenomena swept over the earth. A total eclipse of the sun is an awful phenomenon, and it was scarcely to be expected that in its presence a tremendous problem should be solved at the very first attempt. All the observers found all their laborious preparations of many months culminating in a

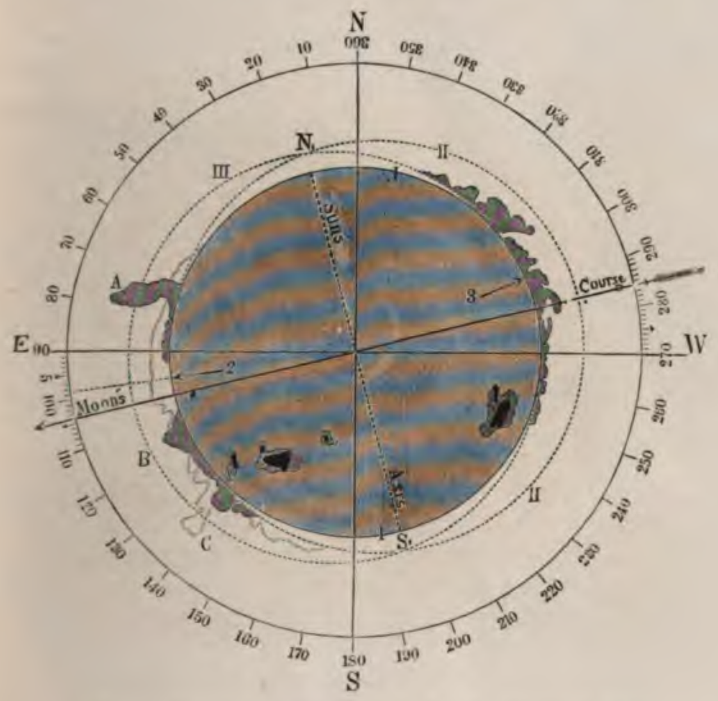

FIG. 29.—Synoptic view of Tennant's photographs. (August 18, 1868.)

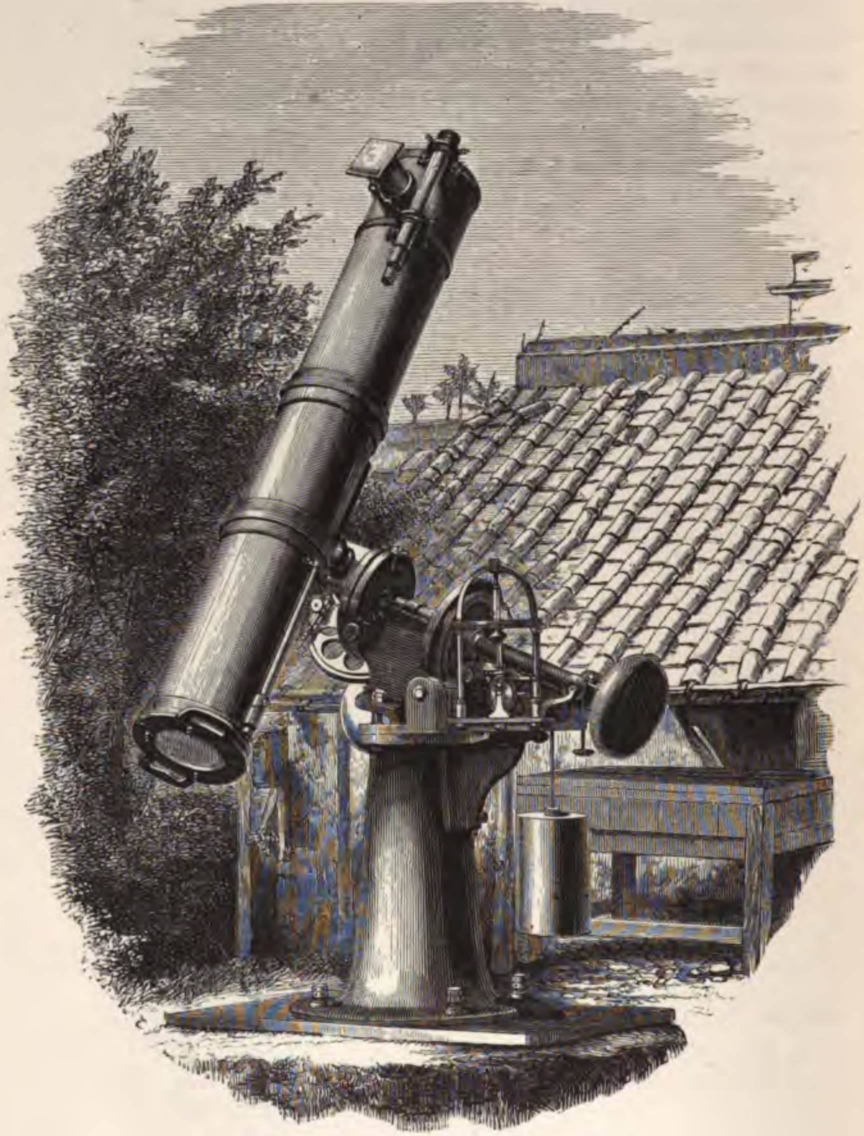

FIG. 30.—Tennant's reflector used for photography.

few minutes, and those minutes rendered part almost of a new existence in a new world by the unaccustomed look of things. The mental tension must have been extreme: the hope of widening the range of knowledge, and the fear of losing a single precious instant, are not calculated to steady either the hand or the eye, and it is no discredit to these men to point out that the results they obtained were discordant as to the positions of the bright lines observed.

Success.

Such was, as I imagined, the condition of things when, on the 20th October, four days after I had received my new instrument—which promised to succeed if any trust was to be placed in principles—I at last saw for the first time the long-wished-for lines; and, in my observatory, the sun shining in all the glory that an English autumn permits, at my leisure, though not without excitement, measured their absolute positions on the solar spectrum itself, both the bright lines proceeding from the prominences and the brilliantly coloured cypher-band proceeding from the sun's edge being spread out before me, allowing an absolute means of determining the position of the former with reference to the latter, all doubt, uncertainty, or error as to their position being rendered impossible—an advantage which the eclipse observers were deprived of, owing to the temporary obscuration of the sun.

Lines visible.

Three beautifully coloured lines of light were visible. Two of them corresponding to C and F in the solar spectrum showed that the famous red flames were composed in part, at least, of incandescent hydrogen gas; that hydrogen gas was present in the atmosphere of the sun in volumes beside which the size of the earth is as nothing, welling up in what may be almost considered tongues of flame to a height of 70,000 and 80,000 miles, now running out into strange shapes and branches, now parting from the lower surface and floating cloudlike in the higher regions. Besides these two lines, which settled the question as to hydrogen, another line was observed near D, which strangely enough, had no dark line in the solar spectrum, corresponding with it. I soon found that by sweeping the slit of the spectroscope along the sun's edge and over the prominences, it was quite easy to determine their outline, the length of the bright line visible giving the height of that part of the prominence on the slit at the time.

Janssen.

Here, then, was all doubt and uncertainty removed as to the position of the lines, and a method discovered of mapping the prominences every day the sun shines, instead of glimpsing them every ten years or so. Immediately after a second letter giving some further details had been communicated to the President of the Academy of Sciences in Paris, a letter was received from M. Janssen, one of the observers of the eclipse, to the effect that the same idea which I had published in 1866, had struck him during the eclipse itself; that he had applied it the next day, and had determined the absolute position of the lines at his leisure, as I did. The fact, which was determined by both of us, that bright lines corresponding to C and F in the solar spectrum appeared in the spectrum of the prominences, as before stated, settled the question as to their nature, for these are the two principal lines given out by hydrogen gas. The spectroscope, therefore, had taught us that the prominences were composed wholly, or in part, of hydrogen—incandescent hydrogen gas, bursting up in tongues of flame and cloudlike masses from the photosphere.

M. Janssen continued to observe the prominences for seventeen days after the eclipse, and before his observations were received it was known that they could not fail to be of the highest importance, for he is a practised observer, has long devoted himself to spectroscopic research, and had a sun not far from the zenith to work upon.[1]

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[1] As this article appeared in the *Times*, a paragraph was inserted here on the question of priority. I omit it from the text, but in justice to M. Faye I introduce it as a note:—

"Here, then, was a nice point of priority to settle. Mr. Lockyer had been the first to conceive the plan and to announce its success, but M

Janssen's detailed work.

The eagerness with which the details of M. Janssen's seventeen days' work in India were waited for may be imagined. In the meantime I had continued to apply the new method; the few observations possible during the months of October and November had opened out a broad expanse of new fields of investigation and speculation which a few months ago were undreamt of. Before, however, we pass to this later work, it will be well to refer to the details of M. Janssen's observations, which arrived in Paris in due time.

M. Janssen's detailed account of his work, consisting of a report to the President of the Bureau des Longitudes, is of very high interest. During the eclipse he does not appear to have been more fortunate than the members of the other expeditions; he is uncertain even as to the number of lines observed, whether five or six, but the uncertainty was not of long duration. M. Janssen was struck with the great brilliancy of the red flames, and the idea occurred to him that—we use his own words—"it might be possible to see them without an eclipse. Unfortunately, the weather, which

Janssen had made the discovery really two months before Mr. Lockyer, one month of the time being taken up in the transmission of the news from India. No doubt we should have heard very much more of this singular coincidence had not M. Faye, one of the most distinguished astronomical members of the Academy, at once attempted a solution of the question of priority in a manner which reflects the highest credit on him for his even-handed justice. He pointed out that, although M. Janssen had really been the first to succeed, Mr. Lockyer had first suggested the idea in 1866, and also that Mr. Lockyer's prior announcement depended really upon M. Janssen's being in India. He did not forget, also, to indicate that Mr. Lockyer's idea had remained long without fruit, and that not only had the observers of the eclipse not given any attention to it, but that the English observers had omitted to make any attempts to see the red flames after the eclipse had happened. He concluded by remarking:—'Instead, therefore, of endeavouring to apportion, and therefore to weaken the merit of the discovery, is it not better to attribute the whole honour, without any reservation, to both of these men of science, who, separated by some thousands of miles, have each been fortunate enough to reach the intangible and the invisible by the method the most astonishing, probably, that the genius of observation has ever conceived?'"

clouded over after the last contact, did not allow me to make any experiment that day. During the night the method and the means of observation were settled in my mind. On the morrow, the 19th, having got up at three, I made the necessary arrangements for the new observations. The sun rose without a cloud, and as soon as it was free from the low mists on the horizon I commenced work."

At last, precisely as it happened to me, a beautiful line flashed up at C, then the existence of another at F was determined. The line at D was not observed by M. Janssen. Having determined these bright lines, M. Janssen's attention seems to have been entirely devoted to the indications, afforded by the varying brilliancy and length of the lines, of the intense action going on, and he declares as a result of his investigations that the prominences "are the seats of movements of which no terrestrial phenomena can give any idea, masses of matter many hundred times larger than the earth changing both place and form in the space of a few minutes."

Later work.

We now come finally to my later work, which branches off curiously enough from M. Janssen's after the discovery and determination of the prominence spectrum.

Between the 20th October and the 5th November, my spectroscope had been rendered more complete (modern science is, alas! very dependent upon complete instruments), and its next revelation startled me as much as the first one had done. The beautiful tell-tale lines added to their former story: not only were the prominences proved beyond all question to be hydrogen, *but the fact, that they were merely local heapings up of a hydrogen envelope which entirely surrounded the sun was established.* The examination of light from all parts of the sun's edge showed that outside the photosphere the prominence spectrum was never absent, and I may add that since the day named, except once in a dense fog, it has never been absent from the field of view of my instrument, whenever I have looked at the

Continuous envelope.

sun—which, thanks to our terrible climate, has happened at intervals, alas! few and far between. This envelope has been named the chromosphere, to distinguish it from the atmosphere on the one hand, and the white photosphere on the other.

Retrospect.

And here, before I go further, a retrospect is necessary. When I commenced my observations, I had no idea that it had ever been suggested that the prominences were part of a continuous envelope. After I had established the existence of this envelope, an examination of Mr. De la Rue's admirable photographs, and of other records, led me to believe that it had really been indicated over and over again, though the indications had been neglected. I have lately, however, been referred by Mr. De la Rue to a report by M. Le Verrier, which I had not previously seen, on the eclipse of 1860, in which the idea of a continuous envelope is distinctly enunciated; and since I have begun this article I have found that such an idea was suggested by Professor Grant before the eclipse of 1851, from a most complete analysis of all the observations made up to that time, and reported in his admirable "History of Physical Astronomy."[1] It is true that Mr. Grant does not refer this third envelope to what we now know to be the right cause, but to him undoubtedly belongs, as far as I now know, the credit of having suggested that the prominences *might be* merely a part of such an envelope, while I have shown they really *are* As I have confessed my own prior ignorance of Mr. Grant's masterly analysis of this matter, I may be permitted to express my surprise that it had been so generally overlooked; so far as I am aware, such an idea was never broached either in connection with the eclipse of 1860 or 1868; whereas, had it been, the continuity of the envelope might have been established easily by observations at properly chosen stations, quite independently of the spectroscope.

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[1] Pp. 395- 401.

In the same chapter Mr. Grant shows also that the early eclipses afford ample evidence that the prominences belong to the sun, although, as we have seen, this fact was not considered to be definitely settled till 1860. To the hydrogen envelope, the existence of which, as an envelope, has now been established by means of the spectroscope, I have, at the suggestion of my friend Dr. Sharpey, given the name of Chromosphere, as it is the region in which all the various and beautiful coloured phenomena are seen.

Here at last, then, is the veil somewhat uplifted—who shall dare to say how little? Under it we see the meaning of the "blood-red streak" observed 162 years ago by Stannyan at Berne,—a meaning finally revealed to us by a process which renders the invisible sensible to the human eye; which allows us, as it were, to feel from world to world. And is this the end? No; the veil is still being uplifted, for modern science moves apace. Though "Ars longa, vita brevis," is, alas! still too true, its truth is not the old truth; it is now becoming a question more of extent than of time. The wondrous cypher-band has other secrets to reveal, and it seems already as if we were about to dwarf our prior efforts to dive into the secrets of the sun. The spectrum is, in fact, a link which binds worlds so closely together, that every terrestrial laboratory is an observatory; and, *per contra*, the sun may teach us chemistry.

At the beginning of my observations, the behaviour of one of the new bright lines was so strange and unexpected, that I was for a time completely puzzled; its message was hard to read, but an alteration in the instrument made the matter clearer. The hydrogen spectrum at the upper *niveau* of the chromosphere was different from the spectrum of the lower level,—precious indications, going far to prove that with patient research we may not only increase our knowledge of the hydrogen spectrum by observations of the prominences, but may arrive at a knowledge of the temperature and density of these circumsolar regions.

CHAP. VIII.

It is not a little singular that a method so clearly indicated in 1866 should have waited till 1868 for its ultimate success, and the coincidence in time of my results with the receipt of the detailed news from India might lead one not in possession of all the facts to imagine that the success really depended upon the eclipse observations.[1] Now, however, that we have the method, it may be safely affirmed that astronomers have, as it were, taken a new lease of the sun, and have work before them for many years to come. And as we have seen telescope give way to camera, and camera to spectroscope, a time will doubtless come when the spectroscope itself will give place to some other yet undreamt-of instrument, with which our children or children's children will investigate the glorious problem of the sun.

The story of the recent work must now for a time be interrupted in order that the fundamental points of the new science of Spectrum Analysis, on which so much of it has been based, may be placed as tersely as may be before the reader.

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[1] See the discussion of this point further on.

THE BIRTH OF SPECTRUM ANALYSIS.

CHAP. IX. THE field of research which we are now about to consider is one which has been opened up so recently that it may be said that the spectroscope is to the men of science of to-day what the telescope was to Galileo and Fabricius. For even at the present time, although immense strides have been made during the last decade, the science of spectroscopy must still be considered in its infancy. And yet, so far as one can see now—it is always very easy to prophesy after the event—there seems very little reason why the recent discoveries should

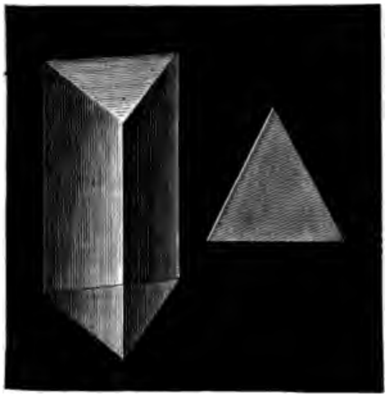

FIG. 31.—Geometrical form of the prism.

not have been made years ago; for nearly two centuries have elapsed since the immortal Newton, following up the researches of Kepler and others, made his classical researches on the action of a prism upon sunlight.

In dealing with the application of spectrum analysis to solar physics, the first part of the story must necessarily in the main consist of an account of the prism and the principles of the spectroscope, and then of a description of the various kinds of spectroscopes which are now employed in solar research; the applications of the spectroscope with regard to terrestrial matters must also be touched upon, so that the full force of the method can be grasped when we come to consider its application to the sun.

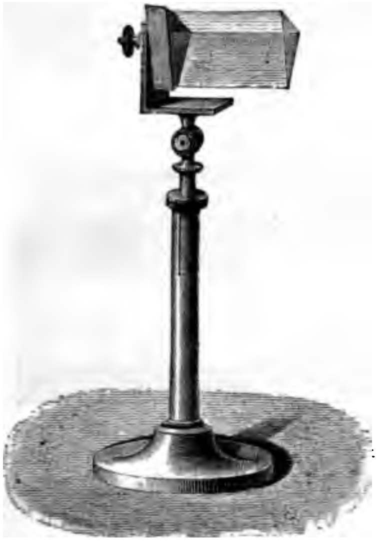

FIG. 32.—Prism mounted on a stand.

Obviously, the first question we have to answer is this, What is a spectroscope? This is answered by saying that a spectroscope is an instrument in which the action of a

What is a prism?

prism or a combination of prisms is rendered most effectual. The next question, then, that arises, is, What is a prism? Figs. 31 and 32 will give a good idea of what is meant by a prism, and little time need be spent in description. Let us rather come to its action.

If a beam of sunlight be allowed to enter a dark room from a round hole in a shutter, it will travel in a straight line from its source; and to make it deviate from this straight line, one of two things must be done. The beam must either be *reflected* or *refracted.*

First, as to reflection. The reflection of light is of very ordinary occurrence, for when light strikes any polished metallic surface, or in fact a surface of any kind, it is more or less reflected by it. The phenomena of reflection are so well known, the use of the mirror or looking-glass being perhaps one of the most tangible, that no detailed reference need be made to them.

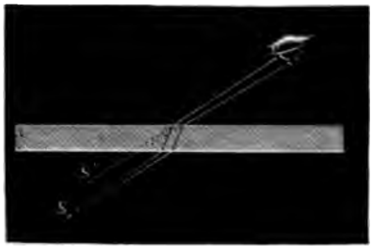

FIG. 33.—Light passing through a plate of glass.

Refraction in media with parallel surfaces.

Let us next interpose in the path of the ray of light a piece of glass, such for instance as window glass, *the surfaces of which are parallel* (Fig. 33). Whether the glass is inclined to the beam or not, it will be observed that the direction of the beam through the room will not be changed: the reason of this is, that when we get the light falling on the glass from the air, then travelling through the glass, and coming into the air again, under exactly the same conditions, what is done at the first surface—and

Refraction in media whose surfaces are not parallel.

something is done, as we shall see presently—is exactly undone at the second, so that we get pretty much the same effect as at first. But now, if instead of having the glass bounded by parallel surfaces, we use a *prism*, the sides of which are *not* parallel, you will see that there is a distinct alteration in the effect produced; the beam is now forced to another portion of the wall altogether. The ray strikes the first side of the prism, and is bent towards the thicker part, or towards a line perpendicular to the surface on which it strikes; and on reaching the second side of the wedge, the ray is again bent in the same

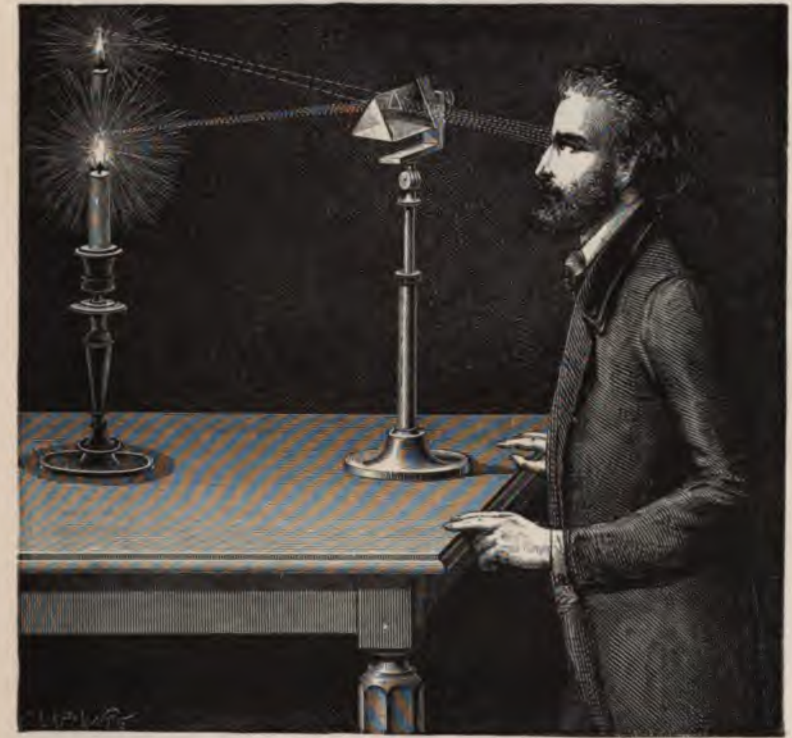

FIG. 34.—Images of objects seen through prisms.

CHAP. IX.

direction, towards the base of the prism; in this case the ray is bent away from the perpendicular to the second surface, and the light emerges from the second surface in a totally new direction. An experiment may easily be tried, which will confirm this. Let a prism of glass be held, with an edge upwards, between the eye and a lighted candle, as shown in Fig. 34; it will be found that the candle cannot be seen; but if the prism be gradually raised, the image of the candle will appear, the amount the prism will have to be raised depending on its angle. Now, we have here obtained a ***deviation*** or ***refraction*** of light,—that is to say, it has been bent out of its course; for we have to look upwards to see the candle.

Kepler's reference to prismatic refraction.

Now this effect and something more had been observed by Kepler, who thus refers to it in his "Dioptrics:"[1]

"*Axioma Sensuale.*

"XVI.

"Colores Iridis jucundissimi oriuntur cum refractio est tanta: idque tam si oculi transpiciant quam si Sol transluceat.

"XVII.

"Sole prisma irradiante tria genera radiorum resultant, Sincerus, Vitri colore, et Iridis coloribus.

"Sit enim F Sol. Is radiet in D. Hic quasi dividitur radii solaris densitas, quæ minima sui parte repercutitur in D I, et anguli, A D I, equali ipsi B D F quo illabitur. Sincerum igitur radium, sed tenuem per D I vibrat in I. Sincerus est, quia in vitro tinctus non est, cujus corpus non ingreditur.

"Potior autem pars de densitate ipsius F D penetrat D et refringitur in D E. In E vero rursum dividitur, ratione densitatis. Potior enim pars transit E, et propter geminam magnam refractionem colores Iridis jaculatur in G.

"Residuum ipsius D E tenue admodum repercutitur a superficie A C in E M: quod si D E paulo obliquius in A E incidit, obliquius igitur in E M refringitur quam hic. Nam si minuas D E A, erit et minuendus M E C, ex lege repercussus. Et sic denique E M in B C rectus incidet, itaque nihil in M refringetur. Cum autem F D hoc pacto bis pertransierit corpus vitri, quippe semel in D E, iterum in E M, exiens recta per M, radium vitri colore jaculatur in K, rectius tamen e regione ipsius A. Nam docemur ex opticis, radios lucidos tingi in mediis coloratis."

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[1] "Dioptrice, sive demonstratio eorum quæ visui et visibilibus propter Conspicilla, hoc est, vitra seu Crystallos pellucidos, accidunt."

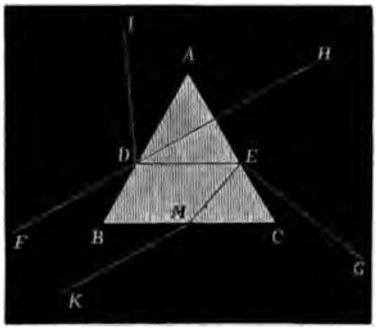

FIG. 35.—Copy of Kepler's diagram.

Refraction only occurs when the incident ray is oblique to the refracting surface.

Now the refraction or bending of light takes place when the ray passes *obliquely* from one medium to another of different density, as from air into water, or from water into air. A simple experiment may be made by passing the beam of light from above into a glass vessel containing water. If the ray strikes the surface perpendicularly, it will be seen that no visible change takes place ; the ray simply proceeds directly into the water without altering its direction. If, however, the beam be allowed to fall on the surface of the water, say at an angle of about 45°, two things may be observed. In the first place a reflection will take place at the surface of the water, that is to say, the light will appear reflected at the surface, and it will be noticed incidently that the angle at which the reflected ray leaves the water is precisely equal to that at which the incident ray strikes

The direction of the ray within the refracting medium is changed.

the surface, thus proving the rule that "the angles of incidence and of reflection are equal:" the second thing to be noticed is that on entering the water the direction of the beam of light will not be the same as it was in the air. In Fig. 36, the ray R I, striking the water at I, instead of proceeding to R′ is deflected or refracted to S; that is, the ray will be bent downwards, or, what is the same thing, towards a line I P, perpendicular to the surface, to a definite extent, depending on the angle of the incident ray. The experiment may be varied by allowing the light to fall on the surface at various angles, when it can be shown that the angle formed by the ray refracted in the water varies according to the angle of the incident ray, and that the angles formed are bound together by a regular law. A

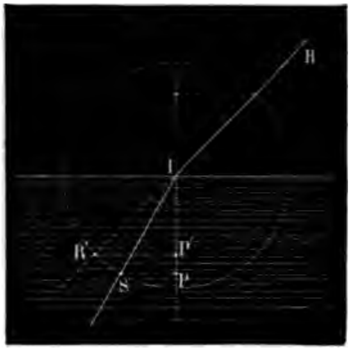

FIG. 36.—Path of the refracted ray.

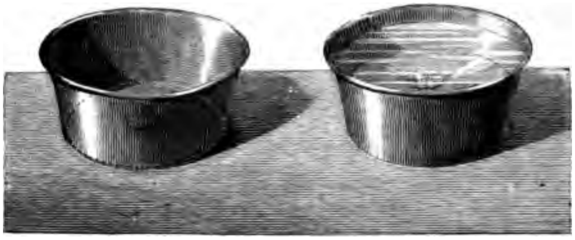

FIG. 37.—Refraction of light. Apparent elevation of the bottoms of vessels.

very instructive experiment is to place a coin at the bottom of a vessel, and then, standing so that the coin is just hidden by its edge, to gradually fill the vessel with water; the coin with the bottom of the vessel will appear to rise, and will become visible, as shown in Fig. 37.

The ray is more bent from its course the denser the refracting medium is.

The amount of refraction varies with the medium employed, and also with its temperature. The effect of different media can be clearly seen by passing a ray of light into a vessel, containing a liquid such as bisulphide of carbon, with a layer of water floating on the top. The ray will be seen to be bent on entering the water, and to be still more bent on passing from the water into the layer of bisulphide of carbon.

We have now to see what takes place when a ray of light enters a piece of glass. We will take first the case of glass with parallel sides. The ray on entering the glass at the upper surface is refracted downwards, as in the case of water, and travels through the glass until it reaches the under surface. Here we have precisely the reverse condition holding,—that is, the ray of light passes from a dense medium to a rarer one; the ray is refracted upwards or away from the perpendicular line, and thus will exactly neutralize the previous refraction, and the beam of light will come out in a direction parallel to its original path, though not quite in the same straight line: as shown in Fig. 33, the ray, instead of proceeding in the direction of S′, proceeds in the direction of S.

There is another effect.

So much for the deviation or refraction resulting from the action of a prism upon light, but there is another effect: the light, which was white on entering the prism, is separated into several colours on leaving it: the candle as seen in the experiment in fact is not white, but is fringed with colours. If we again take our beam of light in the dark room, as in Fig. 38, and allow it to strike on a prism, so placed that its edges are horizontal, and also that the beam enter its obliquely by one of its surfaces, and then receive the image on a screen, we see a band of colours which reminds us

The refracted beam is coloured.

strongly of the rainbow: the lowest colour, if the base of the prism be upwards, will be red, next above orange, passing by imperceptible gradations to yellow, and afterwards green, which then passes through the shades of greenish blue till it becomes a pure blue, then indigo, and finally ends with a violet colour. The transition from one colour to another is not abrupt, but is made in an imperceptible manner, so that it can scarcely be said, for instance, where the yellow ends or the green begins.

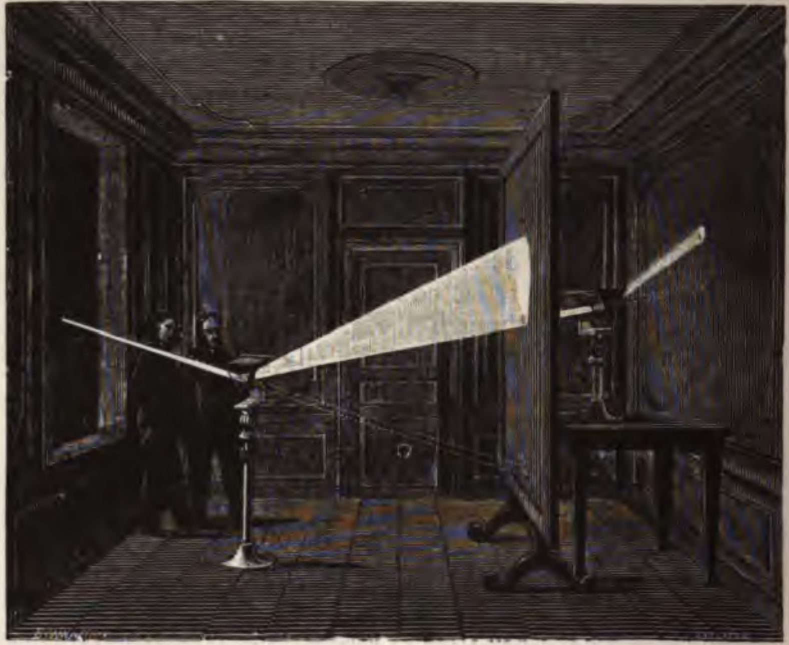

FIG. 38.—Decomposition of light by the prism. Unequal refrangibility of the colours of the spectrum.

The cause of this band of colours, or *spectrum*, as it is called, was first discovered by Sir Isaac Newton; and here we see the birth of Spectrum Analysis. He took one of the colours thus produced—say red, as is shown in the figure—and made it pass through a second prism, receiving the image on a second screen; the image was found to be rather longer, but the colour remained unaltered. This experiment proves that this colour of the spectrum is simple,

The colours of the spectrum cannot be again

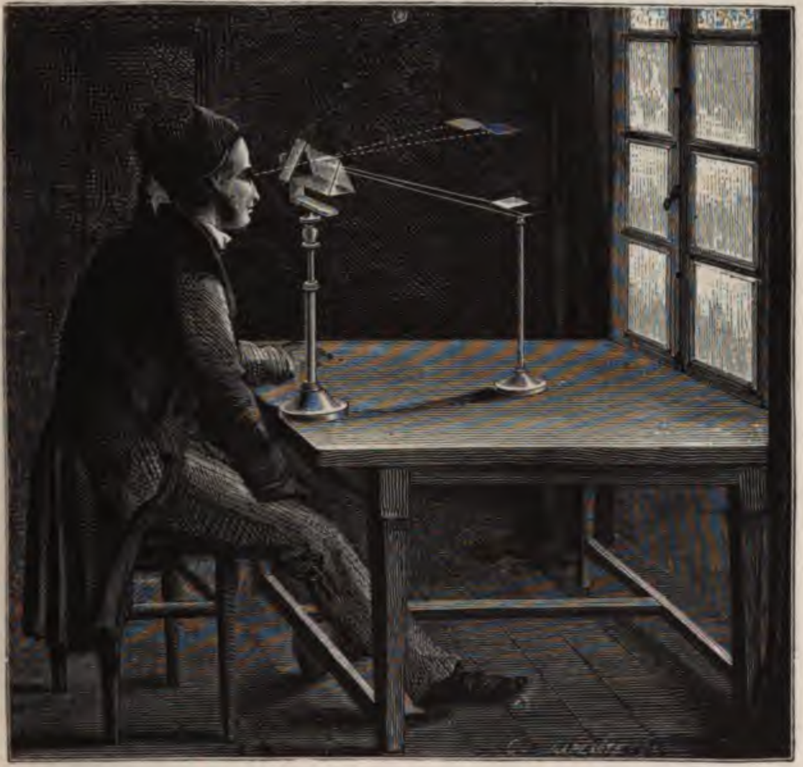

FIG. 39.—Unequal retrangibility of two differently coloured lights.

and the same has been found of all the others. As Newton in his experiment operated with sunlight, the band of colours was in this case called the *solar spectrum.* The rainbow is nothing more nor less than a solar spectrum, caused by refraction in the rain-drops.

broken up by further refraction.

Let us next take two beams of differently coloured light, *The different colours are refracted in different degrees.*

red and blue for instance, and pass them through the same prism. We see that the action of the prism on these two differently coloured beams is unequal; in other words, we get the red beam deflected to a certain distance from a straight line, and the blue deflected to a certain other distance. We see by this experiment that there is a distinct difference in the amount of refrangibility—that the red light is not diverted so far out of its original direction by the prism as the blue. And this leads us to Newton's first proposition, which is this: "*Lights which differ in colour differ in refrangibility.*"[1] This requires no explanation; it may be translated—Lights which differ in colour are differently acted upon by a prism.

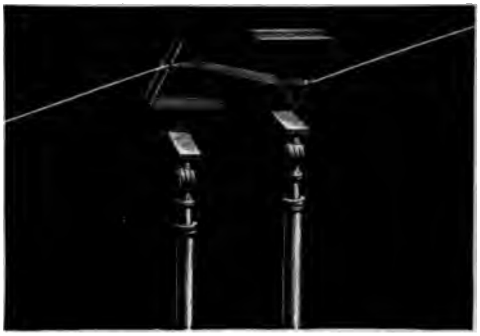

FIG. 40.—Recomposition of white light by means of a second prism.

We now approach Newton's great discovery, which is this: "*The light of the sun consists of rays differently refrangible.*"

Let us take a beam of sunlight this time, and make it pass through a prism, bearing in mind the former experiment: we shall see then that the action of the prism is at once to turn that beam into a beautifully coloured band which reminds us of a rainbow. It was this which Newton did in a dark room, which led him to his important discovery—that *white light is compounded of light of different*

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[1] See Appendix for extracts from Newton's "Optics."

How white light is reproduced.

degrees of refrangibility. But how is it possible to show the truth of Newton's assertion that white light is compounded of these different colours? We can do so by simply placing in the path of the coloured beam another prism placed in a contrary direction, as shown in Fig. 40. In the same way white light may be reproduced by means of a lens (Fig. 41). We see in a moment that we get back white light; for the second prism exactly neutralizes the effect caused by the first, and the ray proceeds as if nothing had happened.

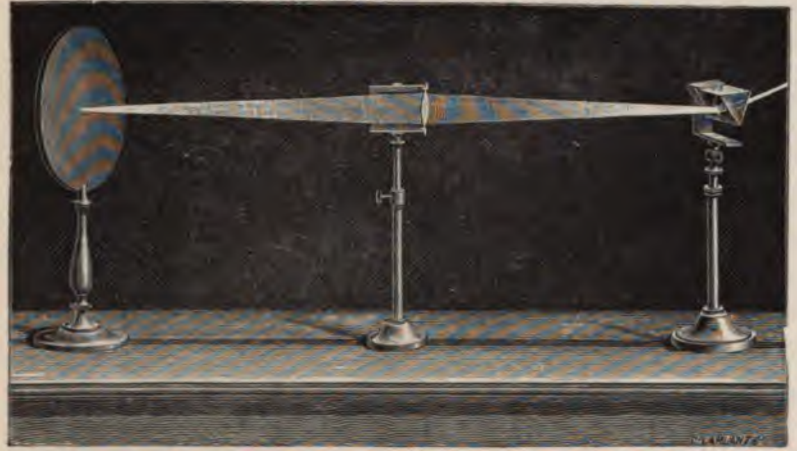

FIG 41.—Recomposition of white light by means of a lens.

This can be shown by an experiment of a different order. If a disc, divided into sectors and coloured with the principal colours of the spectrum, as shown in Fig. 42, be taken, and if it be true that the idea of white light is simply an idea built up by the eye, because we have all these multitudes of light-waves perpetually pouring into it with a velocity that is very much greater than anything which can be translated into words, surely we should get something like this effect also if we were able, by rapidly rotating the disc, to obtain a more or less perfect substi-

Effect of rotating a coloured disc.

tute for white light. The coloured disc being made to rotate rapidly, we obtain something like an approximation to white light. To show that this is really an effect due to the flowing in of light from differently coloured parts of the disc into the eye, and so forming this compound impression which is conveyed to the brain, we can vary the experiment in this wise. Instead of illuminating the disc continuously by the electric lamp, or by sunlight, illuminate it intermittently, by an electric spark; then, although the disc is rotating rapidly all the time, each separate colour is discernible, and the disc appears to stand still. The reason of this difference is, that in one case the rotation of the wheel builds up a compound image in the eye, and in the other case it cannot do so, because the flash of the light is much more rapid and instantaneous than the rotation of the wheel.

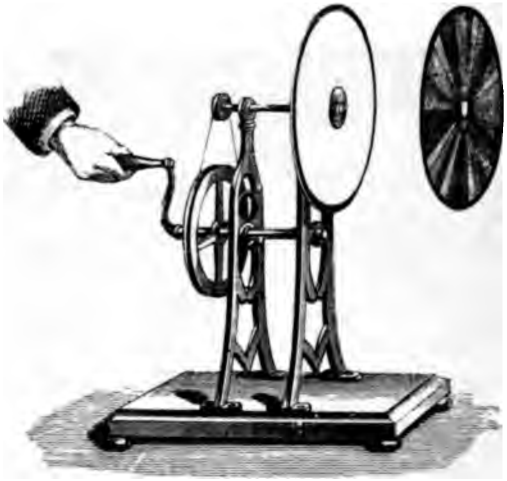

FIG. 42.--Recomposition of white light by means of a rapidly revolving disc, coloured in sectors.

There is one more experiment which can be easily made, to show that all the beautiful colour which we get in nature is really reflected after all, and that if our sunlight, *Effect of monochromatic light.*

instead of being polychromatic—that is to say, compounded of all these beautiful colours—were monochromatic, or of one colour only, the whole expanse of creation would put on a very different appearance from what it does. If, instead of illuminating a diagram, the letters of which are of different bright colours, by the white light of the electric lamp, we illuminate it by a light that only contains one colour—by the yellow light of sodium, for instance—and then look at it, we see that some of the letters upon it are almost invisible, whilst others are very clear, the yellow light only allowing a difference to be seen of more or less depth of shade—there being no difference in colour. But if we allow the polychromatic light of an electric lamp or of the sun to fall upon the diagram, we at once see all the letters in different colours. This experiment feebly indicates the advantage we possess in living in a universe lit by white, or polychromatic, or many-coloured light, instead of light which is merely blue, or yellow, or any other single colour.

Dispersion.

Hitherto we have spoken of refraction. We will now introduce the word *dispersion*, which represents simply a measure of different refractions, or the difference between the bending of the red and the violet rays of light. In an ordinary spectrum, the difference between the red and the violet is the difference of the refractions of those two colours by the prism; and the angle which the red, or yellow, or other colour, forms with the original path of the compound-beam, is called the *angle of deviation* of that colour.

Angle of deviation.

There is one other consideration which we owe to Newton. In his very first experiments, that great philosopher discovered that the quality of the spectrum depended very much on the following consideration:—If we wish to get the best possible effect out of a prism, and the purest possible spectrum, we have so to arrange it that the particular ray which we wish to observe, whether the yellow, the blue, the green, or any other, leaves that prism at exactly the same angle as the incident compound ray falls on it. This angle is termed the *angle of minimum deviation.*

Angle of minimum deviation.

It is very curious, however, that Newton, although he made many experiments on prisms, really omitted one of the most important points; and here again we get an idea of the enormous patience which is necessary in these matters, for we had to wait a century and a quarter before the next essential point was hit upon which has helped us in our study of the solar spectrum. Newton made a round or oblong hole in a shutter for his experiments, but we now know he ought not to have done that; he ought to have made a slit. But this did not come out until 1802, when Dr. Wollaston, by merely using a slit instead of a round hole, made a tremendous step in advance—the real basis of all the modern work which has been done in solar physics by means of the spectroscope. The importance of this is obvious. Suppose we take a cylindrical beam of sunlight and put a prism in the path of the beam, we observe that the spectrum is not a pure one; but if we change the round hole for a slit, we obtain a spectrum of the greatest purity: the red, blue, green, and violet, instead of overlapping and destroying the beauty of the spectrum, show distinctly as simple colours, each one speaking for itself on the screen. By using this narrow slit instead of the round hole which Newton made in the shutter, we got the first idea of the tremendous importance of spectrum analysis; for no sooner had Dr. Wollaston examined the sunlight with the new arrangement, as Newton had done a century and a quarter before with the old one, than he found out that it was not at all as Newton had represented it. Newton told us, in fact, that the sunlight was continuous; that is to say, that the spectrum was one in which there was no break in the light which flowed out to every part of the spectrum, from the extreme red to the violet. When Dr. Wollaston tried the slit, he found, however, that the spectrum, instead of being an unbroken

Wollaston first uses the slit in 1802.

rainbow band of light, was really broken by a succession of fine—beautifully fine—black lines.

Here is an extract from Wollaston's communication to the Royal Society:[1]—

Wollaston's paper.

"I cannot conclude these observations on dispersion without remarking that the colours into which a beam of white light is separated by refraction appear to me to be neither seven, as they usually are seen in the rainbow, nor reducible by any means (that I can find) to three, as some persons have conceived; but that, *by employing a very narrow pencil of light*, four primary divisions of the prismatic spectrum may be seen with a degree of distinctness that I believe has not been described nor observed before.

"If a beam of daylight be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye, at the distance of ten or twelve feet, through a prism of flint glass *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only: red, yellowish-green, blue, and violet; in the proportions represented in Fig. 43. The line A that bounds the red side of the spectrum is somewhat confused, which seemed in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet; but C, the limit of green and blue, is not so clearly marked as the rest: and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which in an imperfect experiment might be mistaken for the boundary of these colours. The position of the prism in which the colours are most clearly divided is when the incident light makes about equal angles with two of its sides. I then found that the spaces A B, B C, C D, D E, occupied by them, were nearly as the numbers 16 23, 36 25. Since the proportions of these colours to each other have been supposed by Dr. Blair to vary according to the medium by which they are produced, I have compared with this appearance the coloured images caused by prismatic vessels containing substances supposed by him to differ most in this respect, such as strong but colourless

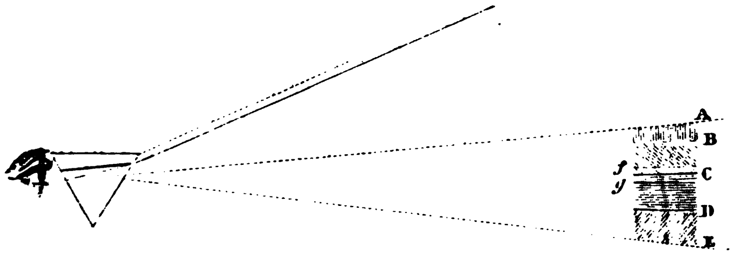

FIG. 43.—The first observation of Fraunhofer's lines.

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[1] Philosophical Transactions, 1802, part i. p. 378.

Wollaston's paper.

nitric acid, rectified oil of turpentine, very pale oil of sassafras, and Canada balsam, also nearly colourless. With each of these I have found the same arrangement of these four colours, and, in similar positions of the prisms, as nearly as I could judge, the same proportions of them.

"But when the inclination of any prism is altered so as to increase the dispersion of the colours, the proportions of them to each other are then also changed, so that the spaces A C – C D, instead of being, as before, 39 and 61, may be found altered as far as 42 and 48.

"By candle-light a different set of appearances may be distinguished. When a very narrow line of the blue light at the lower part of the flame is examined alone, in the same manner, through a prism, the spectrum, instead of appearing a series of lights of different hues contiguous, may be seen divided into five images at a distance from each other. The first is broad red terminated by a bright line of yellow, the second and third are both green, the fourth and fifth are blue, the last of which appears to correspond with the division of blue and violet in the solar spectrum and the line D of Fig. 43.

"When the object viewed is a blue line of electric light, I have found the spectrum to be also separated into several images, but the phenomena are somewhat different from the preceding. It is, however, needless to describe minutely appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain."

Although these lines were observed by Dr. Wollaston, it was not until 1814 that we find them mapped out with the greatest care, to the number of 576, by a German optician named Fraunhofer, whose work was quite independent of Wollaston's; hence they are termed "Fraunhofer lines," the principal ones being lettered A, B, C, &c.

Fraunhofer's work will be gathered from the following extract from his communication to the Munich Academy:[1]

Fraunhofer's paper.

"Into a dark room, and through a vertical aperture in the window-shutter, about 15" broad and 36" high, I introduced the rays of the sun upon a prism of flint-glass placed upon the theodolite; this instrument was 24 feet from the window, and the angle of the prism was nearly 60°. The prism was placed before the object-glass of the telescope so that the angles of incidence and emergence were equal. In looking at this spectrum for the bright line which I had found in a spectrum of artificial light, I discovered, instead of this line, an ***infinite number of vertical lines of different thicknesses.*** These lines are darker than the rest of the spectrum, and some of them appear entirely black. When the prism was turned so that the angle of incidence increased, these lines disappeared, and the same thing

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[1] *Denkschriften der K. Acad. der Wissenschaften zu München*, 1814-15, Band 5, pp. 193–226. Translated in *Edinburgh Philosophical Journal*, vol. ix. p. 296, and vol. x. p. 26, 1823.

Fraunhofer's paper.

happened when the angle was diminished. If the telescope was considerably shortened, these lines reappeared at a greater angle of incidence; and at a smaller angle of incidence the eye-glass required to be pulled much farther out in order to perceive the lines. If the eye-glass had the position proper for seeing distinctly the lines in the red space, it was necessary to push it in to see the lines in the violet space. If the aperture by which the rays entered was enlarged, the finest lines were not easily seen, and they disappeared entirely when it was about 40".

"If it exceeded a minute, the largest lines could scarcely be seen. The distances of these lines and their relative proportions suffered no change, either by changing the aperture in the shutter, or varying the distance of the theodolite. The refracting medium of which the prism is made, and the size of its angle, did not prevent the lines from being always seen. They only became stronger or weaker, and were consequently more or less easily distinguished in proportion to the size of the spectrum. The proportion even of these lines to one another appeared to be the same for all refracting substances; so that one line is found only in the blue, another only in the red, and hence it is easy to recognize those which we are observing. The spectrum formed by the ordinary and extraordinary pencils of calcareous spar, exhibited the same lines. The strongest lines do not bound the different colours of the spectrum, for the same colour is almost always found on both sides of a line, and the transition from one colour to another is scarcely sensible.

"Fig. 44 shows the spectrum with the lines such as they are actually observed. It is, however, impossible to express on this scale all the lines and the modifications of their size. At the point A the red nearly terminates, and the violet at I. On either side we cannot define with certainty the limits of these colours, which, however, appear more distinctly in the red than in the violet. If the light of an illuminated cloud falls through the aperture on the prism, the spectrum appears to be bounded on one side between G and H, and on the other at B: the light of the sun, too, of great intensity, and reflected by a heliostate, lengthens the spectrum almost *one-half*. In order, however, to observe this great elongation, the light between C and G must not reach the eye, because the impression of that which comes from the extremities of the spectrum is so weak as to be extinguished by that of the middle of the spectrum. At A we observe distinctly a well-defined line. This, however, is not the boundary of the *red*, which still extends beyond it. At *a* there is a *mass* of lines forming together a band darker than the adjacent parts. The line at B is very distinct, and of a considerable thickness. From C to D may be reckoned nine very delicate and well-defined lines. The line at C is broad and black like D. Between C and D are found nearly thirty very fine lines, which, however, with the exception of two, cannot be perceived but with a high magnified power and with prisms of great dispersion; they are besides well-defined. The same is the case with the lines between B and C. The line D consists of two strong lines separated by a bright one. Between D and E we recognize about eighty-four lines of different sizes; that at E consists of several lines, of which the middle one is the strongest. From E to *b* there are nearly twenty-four lines; at

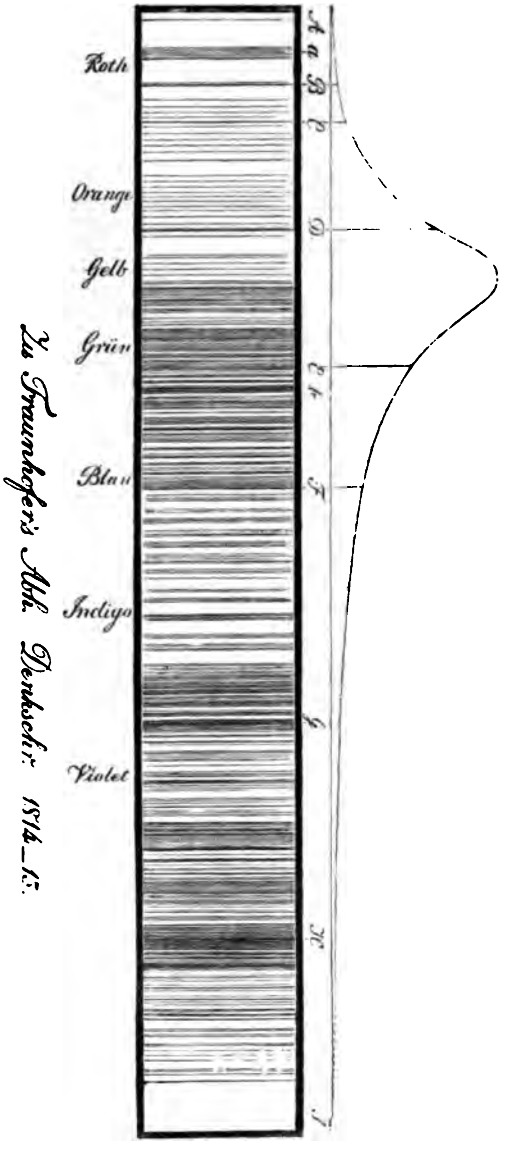

FIG. 44.—Reduced copy of Fraunhofer's map of the lines in the solar spectrum.

Fraunhofer's paper.

b there are three very strong ones, two of which are separated by a fine and clear line; they are among the strongest in the spectrum. The space *b* F contains nearly fifty-two lines, of which F is very strong. Between F and G there are about 185 lines of different sizes; at G many lines are accumulated, several of which are remarkable for their size. From G to H there are nearly 190 different lines. The two bands at H are of a very singular nature: they are both nearly equal, and are formed of several lines, in the middle of which there is one very strong and deep. From H to I they likewise occur in great numbers. Hence it follows that in the space B H there are 574 lines, the strongest of which are shown in the figure. The relative distances of the strongest lines were measured with the theodolite, and placed in the figure from observation. The faintest lines only were inserted from estimation by the eye.

"Various experiments and changes to which I have submitted these lines convince me that *they have their origin in the nature of the light of the sun*, and that they cannot be attributed to illusion, to aberration, or any other secondary cause."

He then points out that in transmitting the light of a lamp through the same aperture we observe a line in the yellow which occupies exactly the same place as D in the solar spectrum. We shall return to this point presently.

The paper then goes on :—

"It is easy to understand why the lines are not well marked, and why they disappear if the aperture of the window becomes too large. The largest lines occupy nearly a space of from 5″ to 10″. If the aperture is not such that the light which passes through it cannot be regarded as a single ray, or if the angle of the width of the aperture is greater than that of the width of the line, then the image of the same line will be projected several times parallel to itself, and will consequently become indistinct, and disappear when the aperture is too great. The reason why in turning the prism we cease to see the lines unless the telescope is lengthened or shortened, may be thus explained :—

"The emersion of the rays in respect to their divergence is similar to their immersion only in the case where the angles of incidence and emergence are equal. If the first angle is greater, the rays after refraction will diverge, as it were, from a more distant point; and if it is smaller, from a nearer point. The reason of this is that the path of the rays which pass nearer the vertex of the prism is shorter than that of those which pass at a greater distance from the vertex. Hence the angles of the refracted rays are not changed, but the sides of the triangles for the emergent rays ought to be in the one case greater and in the other smaller. *This difference ought to vanish if the rays fall in parallel directions on the prism, which is also proved by experiment.* As the violet rays have by the object-glass of the telescope, though achromatic, a focal distance a little shorter than the red rays, we see clearly why it is necessary to displace the eye-glass in order to perceive the lines distinctly in the different colours.

Fraunhofer's paper.

"As the lines of the spectrum are extremely narrow, the apparatus must be very perfect in order to avoid all aberration, by which the lines may be rendered indistinct and even dispersed. The sides of the prism ought consequently to be perfectly plain, and the glass of which the prisms are made ought to have neither scratches nor striæ. With English flint-glass, which is never entirely free of these striæ, we can only see the strongest lines. Common glass, and even the English crown glass, contain many striæ, though they are not always visible to the eye. Those who cannot procure a perfect prism of flint-glass should use a fluid of great dispersive power, such as oil of anise-seeds, in order to see all the lines. In this case the prismatic vessel ought to have its sides perfectly plane and parallel. In general the sides of all the prisms should form an angle of 90° with their base, and this base ought to be placed horizontally before the telescope if the axis of the telescope is horizontal. The narrow aperture by which the light passes ought to be exactly vertical. The reason why the lines become indistinct if any of the conditions now mentioned are neglected, may now be readily understood."

If, therefore, we say that solar spectroscopic inquiry dawned with Newton, certainly the sun began to rise with Wollaston and Fraunhofer.

The solar spectrum, then, as we have said, far from being continuous, is crossed by an almost innumerable number of dark lines, some being fine and others thicker and blacker. But our knowledge of the spectrum is now very much more complete than in Fraunhofer's time, as our means of observing it have been enormously developed.

The improvements of Simms and Swan.

We have now to pass on about thirty years, when Mr. Simms,[1] an optician of world-wide reputation, and Professor Swan,[2] independently of each other, made another very important improvement in the spectroscope. Instead of merely using a prism and observing the slit with the naked eye, they placed a lens in front of the prism, so arranged that the slit was in the focus of the lens. The light which is allowed to pass through the slit is thus turned into a cylindrical beam, and thus the light falls parallel on the full face of the prism, as Fraunhofer suggested it might do (and which method he seems, from the part of his paper near the bottom of page 151 I have italicized, to have used), with great advantage as to the quantity of light used and also to fine definition. Then, instead of having merely the eye to observe the spectrum, as Wollaston had, Fraunhofer's observing telescope is retained, which grasps the circular beam and compels it to throw an image of the slit, which may be magnified at pleasure. We thus see how closely connected are the grandest discoveries with the skill and suggestiveness of those who supply different instruments for our use. In the instrument then devised by Swan and Simms, we have the modern spectroscope. We have next to trace its growth, and the influence of its growth upon the study of solar physics.

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[1] Mem. R. A. S. 1839, vol. xi. pp. 168 and 169. Mr. Simms in this paper (describing the measurement of the refractive index of the optical glass prepared by the late Dr. Ritchie) states, "The only novelty of any consequence in this instrument [the spectroscope employed] is the substitution of a collimator in place of a slit in a window shutter."

[2] Trans. Roy. Soc. Edin. 1847 and 1856.

Effect of the density fo glass on its dispersion.

How then do we in our modern sun-work get beyond the results achieved by Fraunhofer? The reply to this question in the main is that *we employ more dispersion*, but other points have also been alluded to. Let us consider the question of dispersion first. As we have already seen, dispersion is the measure of the difference of the refrangibilities of the variously coloured rays. If we take a prism which appears like an ordinary one, but really is composed of several layers of different kinds of glass, arranged horizontally, and pass a beam of light through it, the beam will be differently acted upon by the various layers, and we shall get a difference in the dispersion. We shall have several distinct spectra, showing that there is something in the different layers of which this prism is composed which turns the light out of its path, and which disperses it more in some cases than it does in others. The cause of this is the different density of the glass composing each layer: some kinds of glass are nearly twice as heavy as others. It is a very natural conclusion, therefore, that the heavier and denser glass should have a stronger action on the light, should give a wider spectrum, and thus enable us to study it better, than the lighter glass does—and fortunately we are not limited to glass; for if we were, we should not be able to go so far in these inquiries as we do. So that, if we want great dispersion, we must use heavy glass, or leave glass behind altogether, as amongst the liquids we find some, such as bisulphide of carbon, which give even a greater dispersion than the densest glass. There is another consideration. The dispersive power and refractive power depend not only upon the density of the glass, but on the refracting angle of the prism. If a beam of light is sent through two prisms of unequal angles, the effect is extremely distinct. Thus, if we take one prism with an angle of 20°, and another with an angle of 60°, the larger angle gives a much greater deviation and dispersion. And now a third important point. There is no reason why we should not employ many prisms, and in practice this is done. First, then, we have a single prism of a dense substance; by increasing the angle we get increased dispersion, and then we get it still further increased by adding another prism, and so we go on, adding prism after prism, until we get to a large number of prisms arranged in the best possible manner for the light to be successively dispersed by each of them, until at last we get a dispersion of such an enormous amount, that the spectrum of the sun, as mapped by Kirchhoff and Bunsen, is several yards in length, although it is nothing but a succession of images of one of the finest slits which our best opticians are able to make.

Effect o the refracting angle of the prism.

We have before seen that our knowledge of the solar spectrum depends first of all on Newton's work with the prism in 1675, and on the fact which Newton found out incidentally, that it is important that the prism should be used at the angle of minimum deviation. We then get the slit added by Wollaston in 1812; then the collimating lens added by Swan and Simms, about 1840; and now we have arrived at the spectroscope improved and modified as an instrument, until at last we get spectroscopes so arranged that *the glass is of the finest possible material, and the densest possible, the angle the largest possible, and the number of prisms as great as possible.*

Reflection from surfaces of prisms.

There is a consideration of great importance which comes in here. If the angle of a prism be large, a ray of light travelling from one prism to another enters the second at an extremely small angle, under which circumstances a large amount of light is reflected, but still it is not better to use a greater number of prisms of a smaller angle than a smaller number of a larger one—in fact, an angle of about 60° is the one generally chosen Mr. Grubb, however, makes *compound* prisms in which the dense flint has an angle of 90° or upwards. With this angle the light would not enter the prism at all. In order to avoid this and still render available the larger dispersion, a prism of crown glass is cemented on to each face of the prism of dense glass, with its refracting angle in the opposite direction to that of the large prism. These are of such angle that their outer surfaces would if produced intersect at an angle of about 60°. The light is thus enabled to enter the combination at an angle at which little is lost.

Again, it should be clear that in spectroscopes of many prisms it is essential that there should be some arrangement by which each part of the spectrum should be observed with each prism at the angle of minimum deviation for that ray. This may be done in many ways.

A very perfect slit is required.

Another consideration, besides the purity of the glass, is the perfect figure of the slit. It might be imagined that the slit of a spectroscope was perfectly easy to make; but, judging by the results of the manufacture, it is extremely difficult, for a perfect slit is still very rare, the best being made by Steinheil of Munich. Spectroscopes are now constructed with one, two, four, seven, or more prisms, the number depending on the purpose for which they are to be employed. The instrument shown in Fig. 45 may be called a chemical spectroscope, for an instrument of this kind is now almost as important and essential in a chemical laboratory as a balance. A spectroscope with two prisms is shown in Fig. 46; this is used in cases when rather more dispersion is desired than can be obtained with the one-prism instrument. When, however, any accurate and elaborate work has to be done, such as in carrying out

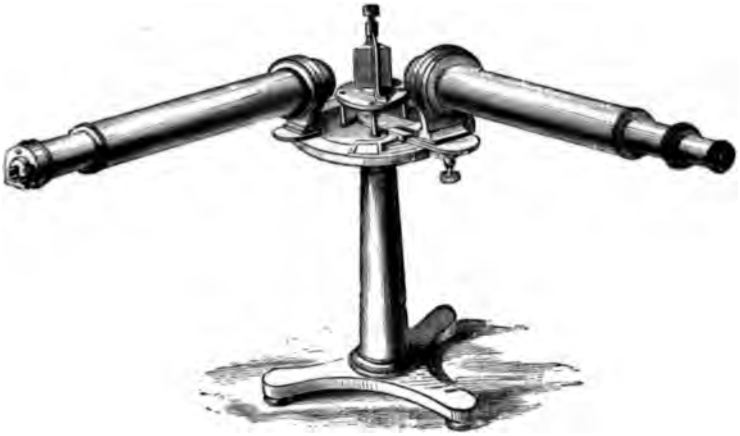

FIG. 45.—Chemical or student's spectroscope.

many investigations, more prisms have to be employed. The engraving given in Fig. 47 represents an instrument

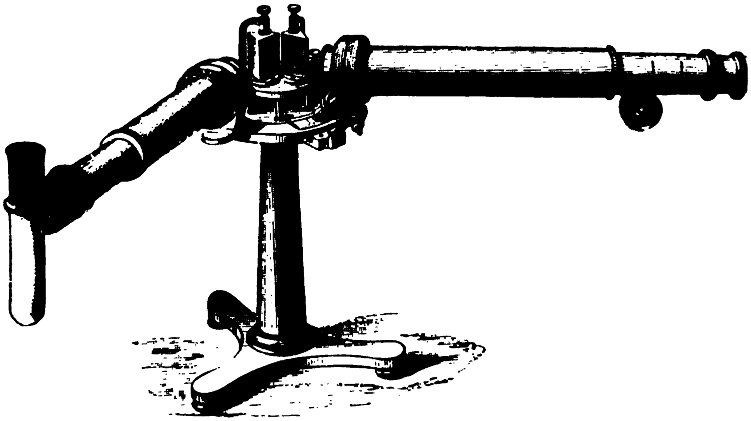

FIG. 46.—Spectroscope with two prisms.

which historically is extremely interesting, as being the one with which Kirchhoff made his most elaborate and

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accurate maps of the solar spectrum; it is furnished with a battery of four large prisms, which give an enormous deviation and dispersion. Fig. 48 represents the simple instrument used by Kirchhoff and Bunsen in their earlier

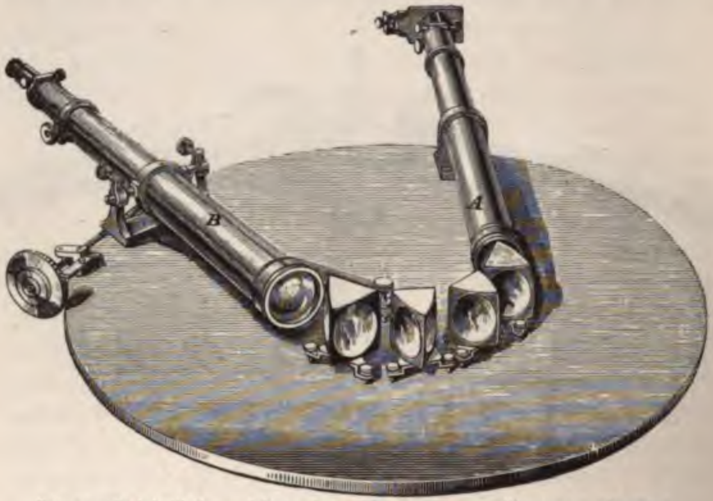

FIG. 47.—Steinheil's form of four-prism spectroscope. A, collimator; B, observing telescope.

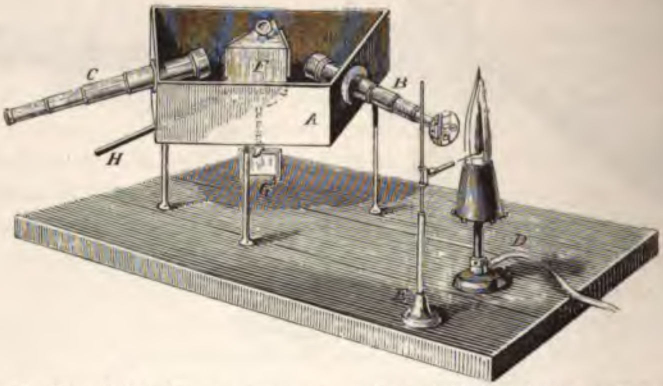

FIG. 48.—Bunsen's first form of spectroscope. A, box to hold the prism; B, collimator; C, observing telescope; D, Bunsen's burner; E, holder; F, prism; G, reflector; H, handle to move prism.

inquiries. Spectroscopes of many prisms can only be employed in the case of strong lights, such as that of the sun or the electric arc, as the light is much dispersed or spread out, and much is lost by reflection.

Description of a spectroscope.

As the principle of construction is almost the same in all kinds of spectroscopes, it will be well to commence with a description of the simplest form, namely that with one prism, as shown in Fig. 49. It will be seen to consist of a circular table, supported by a pillar and three legs, carrying three lateral tubes: the right-hand tube is called the collimator, and holds at its outer extremity the fine slit, the width of which can be regulated to a nicety by a micrometer screw; the other end of the collimator is furnished

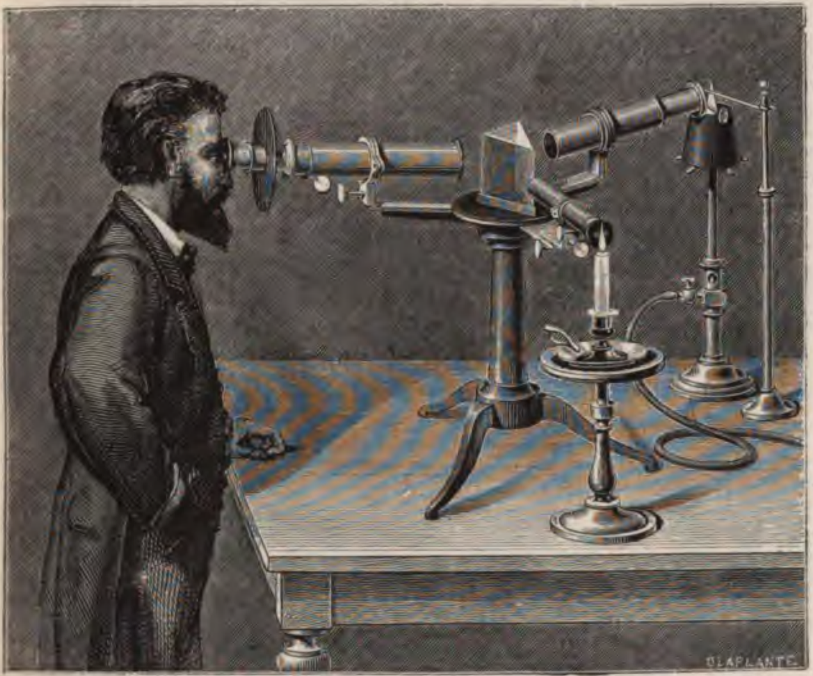

FIG. 49.—Spectroscope with reflected scale.

with a lens, which serves to collect the rays of light coming from the slit, and to render them parallel before falling on the prism in the centre of the table. The prism is so placed and fixed by a clamp that the light entering the slit from the source of light, shown in the figure as a gas lamp, strikes it and leaves it at what is called the ***angle of minimum deviation***, a term which has already been explained; after passing through the prism, in which the light undergoes both deviation and dispersion, the spectrum is observed by the telescope on the left, which is simply a small astronomical telescope of low magnifying power.

Methods of measuring the positions of the lines.

There are two methods of measuring the position of the lines in spectra. The telescope may be attached to a movable arm, which can be directed to any part of the spectrum that may be required; and the outer edge of the circle along which the telescope moves, may be graduated with an accurate scale of degrees, which can be divided with more or less minuteness, according to the precision in the exact position of the dark lines, &c. in various spectra required. In this method the line to be measured is brought into the centre of the field of view of the observing telescope, and the position of the telescope read off. Of course if the line measured is situated in the red end of the spectrum, the telescope will be in a different position from that it would occupy if the line were in the blue end. The second method of measurement may be gathered from Fig. 49. It consists of a short tube carrying at its outer extremity a small photographic scale, which is illuminated by a candle flame; the beam passing from the photographic scale is rendered parallel and thrown on the surface of the prism by means of a lens in the tube carrying the scale, and is reflected by the last surface of the prism up the observing telescope, so that it is seen as a bright scale on the background formed by the spectrum under observation.

By covering up one part of the slit by a reflecting prism, it is possible to observe the spectra of two light-sources at the same time. The *modus operandi* will be gathered from the accompanying woodcuts.

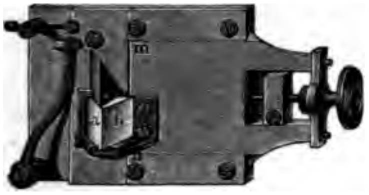

FIG. 50.—Steinheil's slit, showing reflecting prism.

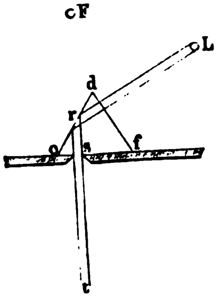

FIG. 51.—Path of light through reflecting prism and into the slit. F, light observed directly; L, light observed by means of the prism; *c*, *d*, *f*, reflecting prism; *s*, slit.

We have now an idea of the action of the simple prism. Another kind of prism, which differs from the simple one very much as the achromatic telescope differs from the non-achromatic one—which was the first attempt made at an instrument for astronomical observation—is next to be noticed. The object-glass of a telescope, as now constructed, consists of two lenses made of different kinds of glass. Of course, we have dispersion and deviation at work in both

Direct-vision prism.

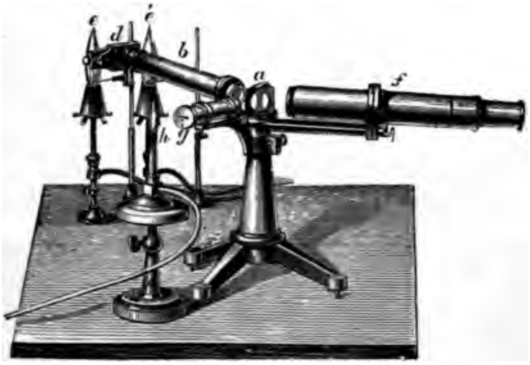

FIG. 52.—*e*, light-source observed directly: *e'*, light-source observed by means of the reflecting prism.

these kinds of glass, but the lenses are so arranged, and their curves are so chosen, that, as a total result, the deviation is kept while the dispersion is eliminated, so that, in the telescope, we have a nearly white image of anything which gives us ordinary light, although it is by the deviation alone that we are enabled to get the magnified image of that object. So also in the spectroscope we have an opportunity of varying the deviation and the dispersion. By a converse arrangement we can keep the dispersion while we lose the deviation; in other words, we have what is called a *direct-vision spectroscope.* If we take one composed of two prisms of one kind of glass which possesses a considerable dispersive power, and three prisms of another kind which does not disperse so strongly, arranged with their bases the opposite way, the deviation caused by the two prisms in the one direction will be neutralized by the deviation of the three prisms in the opposite direction; whilst the dispersion by the two prisms exceeds that which is caused by the three prisms in the opposite direction; the latter dispersion, therefore, will neutralize a portion only of the dispersion due to the two prisms. The final result is that there is an outstanding dispersion after the

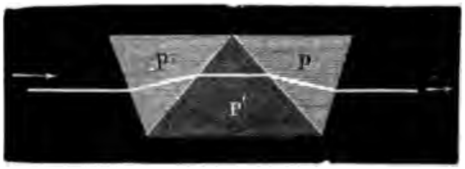

FIG. 53.—Direct-vision prism with three prisms. P, crown; P', flint.

Different effects of glass of different density.

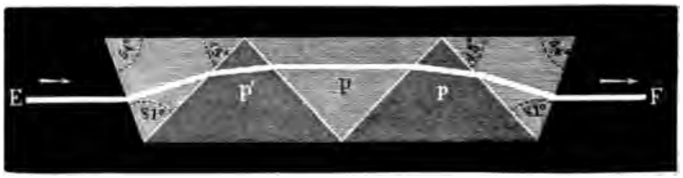

FIG. 54.--Direct-vision prism with five prisms.

deviation has been neutralized, so that when we want to examine the spectrum of an object we no longer have to look at it at an angle. We have an opportunity, by this arrangement, of seeing the spectrum of an object by looking straight at the source of light: in the application of spectrum analysis, especially to the microscope and telescope, this modification—due to M. Janssen, the well-known astronomer, who was the first to bring it into general notice—is one of great practical importance, so that in any research which does not require excessive dispersion, this direct-vision arrangement is getting into common use.

Detailed description.

We must now go a little more into detail, principally with regard to the spectroscopes used in connection with telescopes for work on the solar and other spectra. One class of spectroscopes thus applied to telescopes is arranged

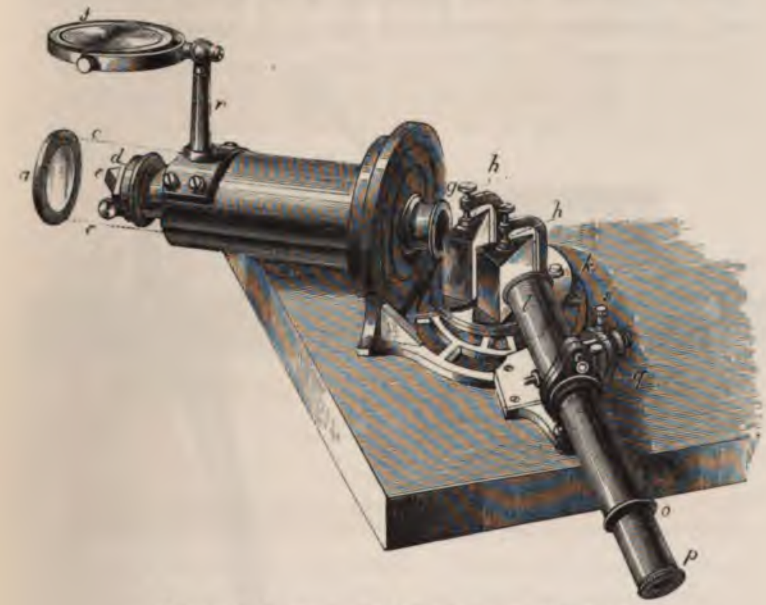

FIG. 55.—Telespectroscope of small dispersion. (Huggins.)

for observing the spectra of the sun *when great dispersion*

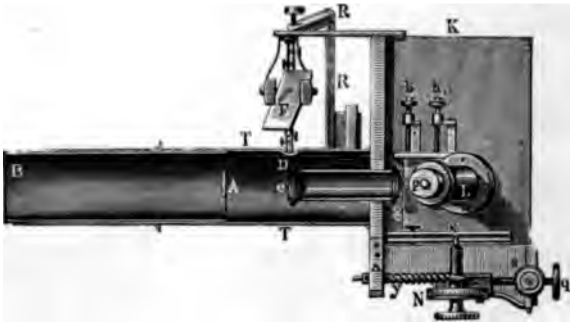

FIG. 56.—Side view of spectroscope, showing—F, the arrangement by which the light from a spark is thrown into the instrument by means of the reflecting prism *e*, by a mirror F. (Huggins.)

is not rcquired: these, therefore, serve at the same time for observation of the spectra of stars, nebulæ, &c., and

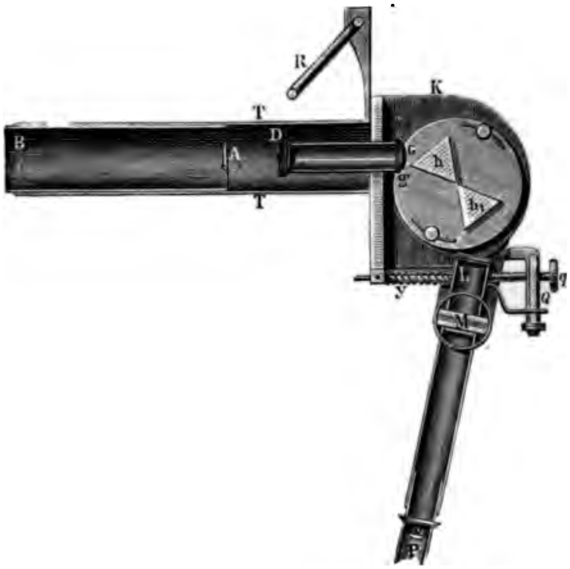

FIG. 57.—Plan of spectroscope. T, eye-piece end of telescope; B, interior tube, carrying A, cylindrical lens; D, slit of spectroscope; G, collimating lens; *h h*, prisms; Q, micrometer. (Huggins.)

another with a much greater dispersive power for observing the spectrum of the sun specially. In both spectroscopes the arrangements employed are similar, and include those of the instruments that have been already described, but there are others necessitated by the attachment to the spectroscope. The spectroscope is attached to the eye-piece end of the instrument, and the image formed by the telescope is received on the slit plate. A finder on the side of the telescope is used which enables the image of the star to be brought on the slit of the spectroscope; while, in the case of the sun, its image can be thrown on the slit screen without such aid, and any part of the image may be brought on the slit by mere inspection. Arrangements are necessary in the case of the star spectroscope (1) for widening out the spectrum—this is done by a cylindrical lens; and (2) for obtaining a spectrum of comparison—this is done by reflecting into the instrument the light of the vapours to be compared, rendered incandescent by an electric spark. Spectroscopes of this class are also made with direct-vision prisms, as in the accompanying one used by Father Secchi.

Attachment to telescope.

Necessities of a star spectroscope.

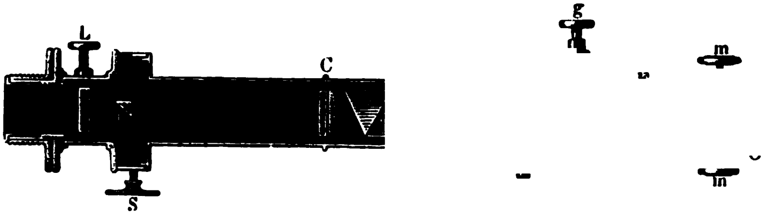

FIG. 58.—Direct-vision star spectroscope. (Secchi.)

In the spectroscopes in which great dispersion is not required, the number of prisms, and the consequent deviation and dispersion, are small. The accompanying woodcuts will make their detailed construction quite clear. In the case of spectroscopes used only for the sun, the deviation and dispersion required are large, the deviation amounting to over 300°; that is to say, the ray of light is bent through almost a complete circle; the light from stars is dim, and many prisms cannot be employed to widen out the spectrum, but, in the case of the sun, there is light sufficient to give us a bright spectrum after it has been enormously dispersed.

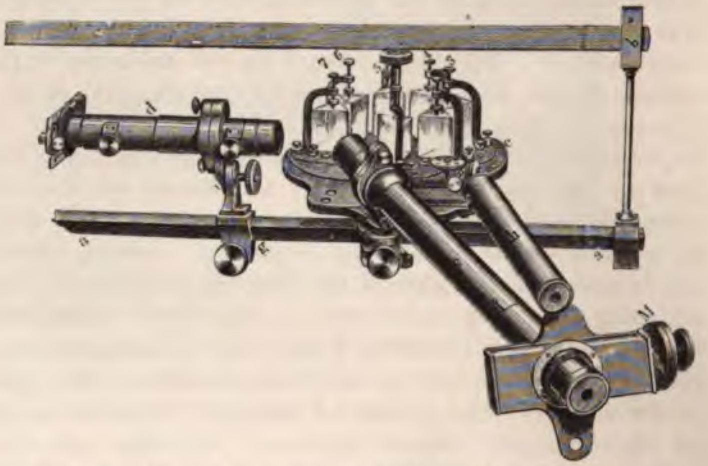

FIG. 59.—Sun spectroscope. *d*, collimator; *e*, observing telescope; *h* and M, two micrometers; 1, 2, 3, 4, 5, 6, 7, prisms.

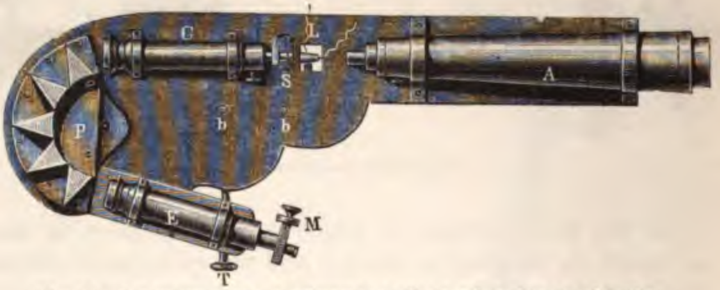

FIG. 60.—Sun spectroscope. A, telescope; S, slit; P, prism plate; E, observing telescope; M, micrometer.

Figs. 59, 60, and 61 show very powerful spectroscopes to be attached to the telescope for observing the spectrum of the sun. One peculiarity of the instrument in Fig. 61 is that the ray of light, having passed once through the lower part of the train of prisms, is received by a right-angled prism,[1] which totally reflects the light twice, sending the ray of light back through the upper part of the same prisms, when it is again refracted; we thus have, by using these prisms, the same effect as if thirteen prisms had been employed. The ray of light enters the instrument by the lower tube, and after passing first through the lower half of the prisms, and back through the upper half, is received in the upper tube, and reflected upwards for convenience of observation. These

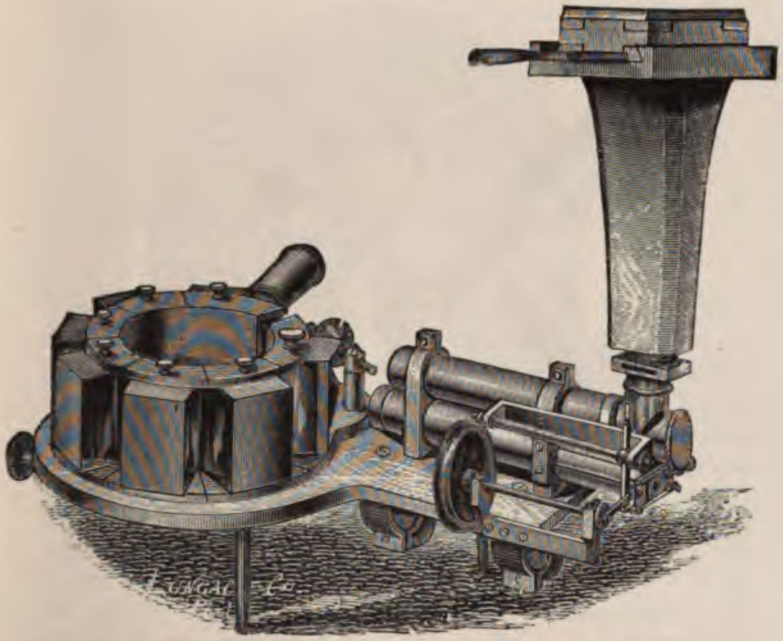

FIG. 61.—Young's sun spectroscope, arranged for photography.

Automatic arrangements.

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[1] I had a spectroscope with a prism of this kind constructed in 1869, and afterwards the same idea occurred independently to Professor Young, as also that of mounting the prisms on a spring, which had been suggested to me by my friend Mr. G. W. Hemming, and introduced into the spectroscope to which I have referred.

prisms are so arranged, that whatever part of the spectrum is being observed, they are always at the angle of minimum deviation for this part of the spectrum—a very important

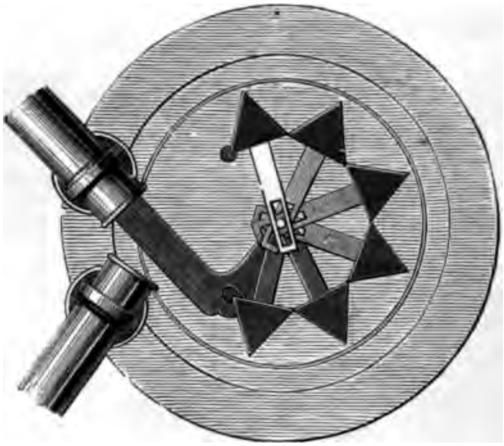

FIG. 62.—Automatic arrangement for securing the minimum deviation of the observed ray.

point, as we have seen. This is done either by attaching the prisms to a spring of ebonite or gun-metal moving on a fixed point near the first prism of the series, as in the arrangement shown in Fig. 61, or each prism may be attached to a radial bar acting on a central pin, as shown in Fig. 62.

Other extensions of the use of the spectroscope in the new method will be alluded to in the sequel.

RADIATION AND ABSORPTION.

Now that the construction and use of the spectroscope have been fully gone into, we come to the principles on which the science of spectrum analysis depends. We first saw Newton founding this new science, by using a hole in a shutter, admitting a beam of sunlight, and analysing it by means of the prism; then we discussed Wollaston's and Fraunhofer's substitution of the slit, and instantly we heard of dark lines in the *solar* spectrum, and bright lines when other light-sources were examined. The foundation of *solar* chemistry has resulted from the explanation and correlation of these dark and bright lines. To get at the *principles* we must first deal with the different modes in which light is given out or radiated by various bodies under different physical conditions—with, in fact, the *radiation* of light.

Effect of heat on a platinum wire.

Now, if we take a platinum wire and heat it to redness, and examine by means of the spectroscope the light emitted, we shall find that only red rays are visible; then if the wire be gradually heated more strongly, the yellow, green, and blue rays will become visible, until finally, when the wire has attained a brilliant white heat, the whole of the colours of the spectrum will be present. If I burn a piece of paper, or a match, or ordinary coal-gas, I get a white light: in fact, if I raise any solid or liquid to a

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Continuous spectrum.

state of incandescence or glowing heat, I get exactly that same sort of light, which will give always ***a continuous spectrum.*** The best method of showing this fact before a large

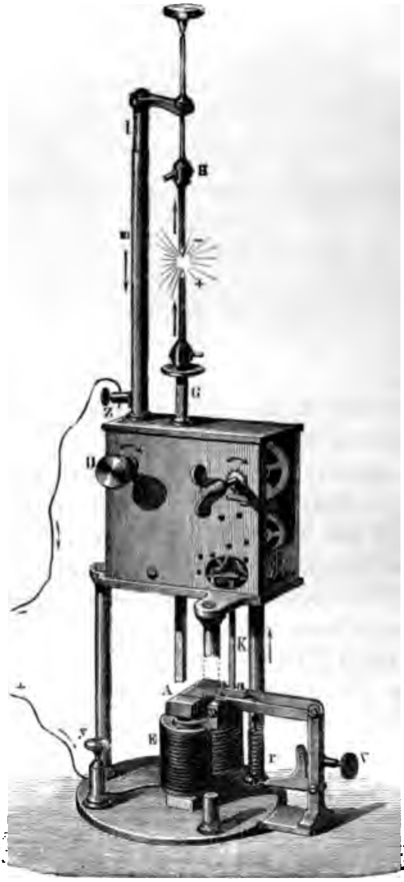

FIG. 63.—Electric lamp. *y*, *z*, wires connecting battery of 50 Grove or Bunsen elements; G, H, carbon holders; K, rod, which stops a clockwork movement, which when going makes the poles approach until the current passes; A, armature of a magnet which by means of K frees the clockwork when not in contact; F, electro-magnet round which the current passes when the poles are at the proper distance apart, causing it to attract the armature A.

audience is to use an apparatus called the electric lamp, and to pass the current of electricity through two carbon points, which are intensely heated by their resistance to the passage of the current. The spectrum obtained from these points, by means of the dispersion of two bisulphide of carbon prisms, is quite continuous from end to end; that is to say, there are no breaks, such as those Fraunhofer saw (Fig. 44), where the black lines represent the breaks in the solar spectrum which are called the Fraunhofer lines.

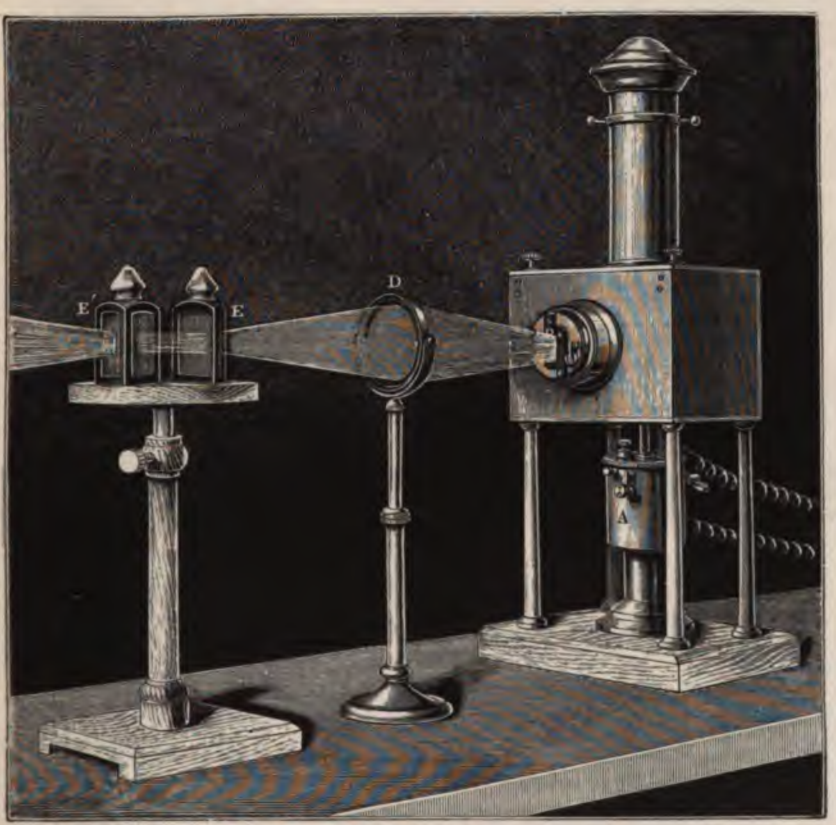

FIG. 64.—Electric lamp arranged for throwing a spectrum on a screen. D, lens; E E', bisulphide of carbon prisms.

First conclusion.

Let us then consider this fact established; namely, that *solid or liquid bodies, when heated to a vivid incandescence, give a continuous spectrum without bright lines.* Under these circumstances the light to the eye, without the spectroscope, will be white, like that of a white-hot poker; if the degree of incandescence is not so high, the light may only be red, like that of a red-hot poker. But so far as the spectrum goes—and it will expand towards the violet, as the incandescence increases, as before stated—it will be continuous. This is fact number one.

Discontinuous spectrum.

Now, instead of observing the spectrum of these solid, white-light-giving carbon points or the light from an ordinary gas flame, let us examine the spectrum of a light-source which is coloured. If, for instance, I burn some coloured fire, such as the red fire of our pyrotechnic displays, on examining such a light by means of the spectroscope, we might expect that in the case of red fire we should obtain the red end of the continuous spectrum; that on burning green fire we should see the green portion of the spectrum, and so on. But this is not so; *we find that the background of the spectrum is dark or nearly so, and that we have certain localizations of light or bright lines in different parts of the spectrum.* Now, the differences in colour are accompanied by differences in the spectra. We have something very different from the continuous spectrum we had before; we have established a difference between a solid or liquid body, which gives us a continuous spectrum, and a vapour or gas which colours a flame, for the spectrum of such a flame contains bright lines.

Spectrum of a vapour obtained.

Flame spectra.

In these instances, then, the spectra consist of lines which are located in different parts of the spectrum. Let us next burn some sodium in air, and then examine the spectrum of its vapour; or, better still, let us place some sodium, or a salt of this metal, such as table salt, in a gas flame which is consuming a mixture of air and gas, in a burner known under the name of a Bunsen's burner, the bluish flame of which is due to the complete combustion

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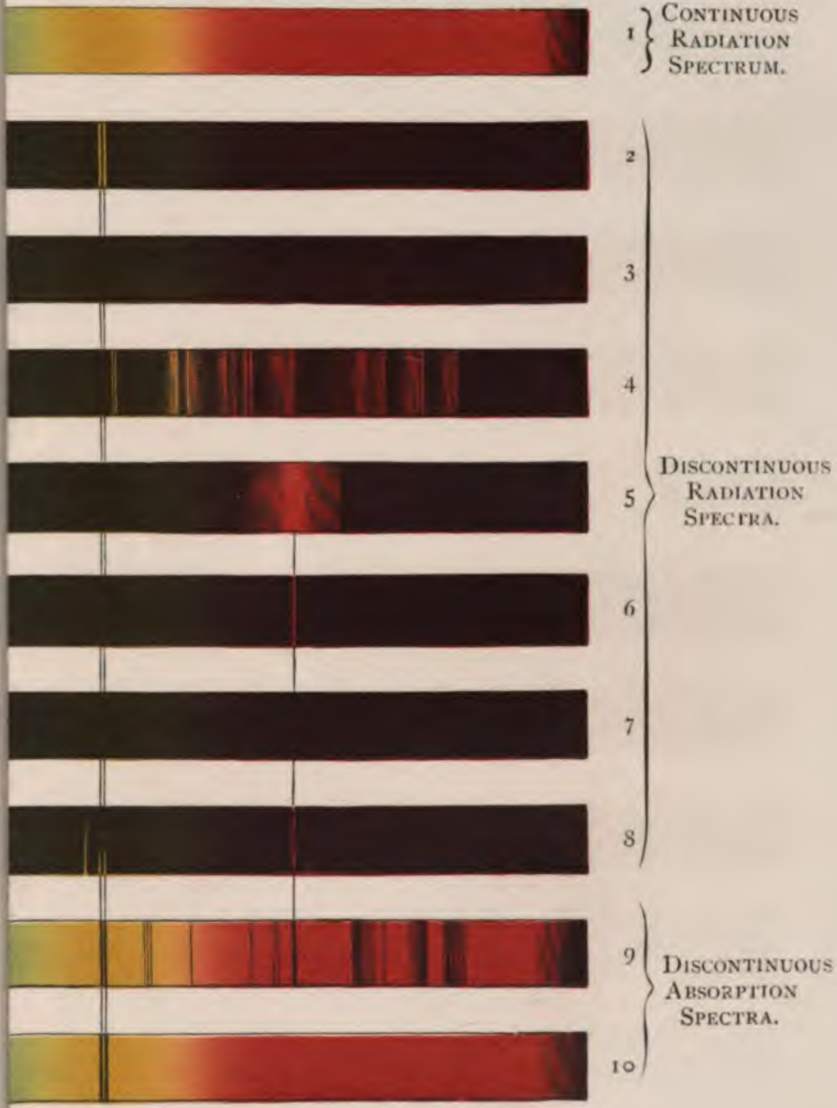

t dense Hydrogen, showing broad lines (p. 177 & 557).
rogen, showing fine lines (p. 177).
icating the presence of rare gases or vapours.
rs which form the Chromosphere of the Sun, showing by
he vapours ascend to different heights.

us Spectrum by Sodium Vapour (p. 19)2.

Bunsen's burner.

due to the greater supply of air from the holes at the bottom. The flame immediately becomes of an intense yellow colour, due to the vapour of sodium. In this we have further evidence of the connection between the colour of the light which we get from a vapour and the spectrum of that vapour. It is usual to place the salt to be examined in a platinum spoon, and insert it in the flame ; but the utmost constancy is insured by adopting an arrangement of Mitscherlich's, shown in the accompanying drawing (Fig. 68), in which a platinum wick is kept continually moistened by a solution of the salt, generally the chloride, the spectrum of which is required to be examined. It will be imagined *à priori*, from what has been already said, that, as in the case of sodium vapour, the colour of the flame is orange, the line of the vapour will appear in the yellow or orange part of the spectrum, and we shall not be mistaken. It is seen, in fact, on examining this flame with a spectroscope, that the spectrum consists of a brilliant yellow line upon an

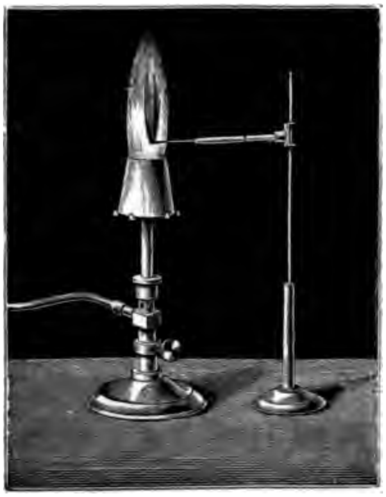

FIG. 65.—Coloured flame of salts in the flame of a Bunsen's burner.

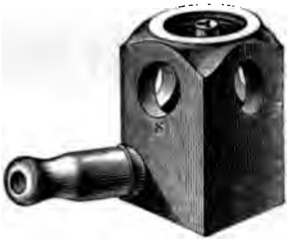

FIG. 66.—Base of ordinary form of Bunsen's burner, showing holes to admit air.

Mitscherlich's arrangement

Spectrum of sodium vapour.

almost black background; if, however, the flame is observed by means of a very narrow slit, this line will appear double: it really consists of two extremely fine lines which are very close to each other.

Lithium vapour.

Let us pass on to another substance, and take some lithium instead of sodium. A brilliant carmine-tinted flame is seen, which on examination by the spectroscope is found to give a spectrum consisting of one splendid red, and a fainter orange line. Potassium gives a violet-coloured flame, and yields in the spectroscope a red line and a violet line. If, again, we take a salt of strontium, one of the ingredients in red fire, it colours the flame crimson, and by the eye the flame can scarcely be distinguished from the colour of the lithium flame, but in the spectroscope there is no possibility of doubt: the spectrum of strontium consists of a group of several lines in the red and orange, and a fine line in the blue end of the spectrum.

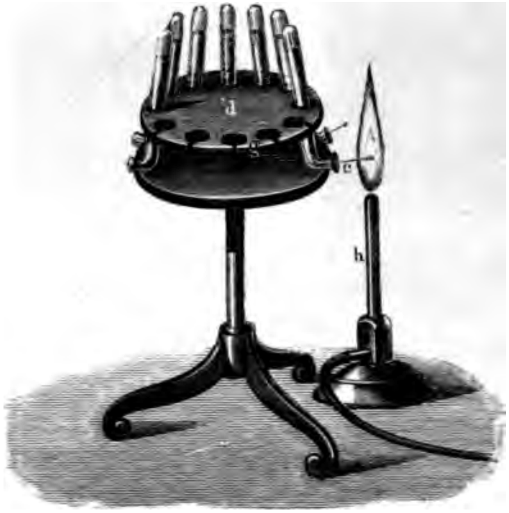

FIG. 67.—Mitscherlich's arrangement for flame spectra. *c*, wick; *h*, Bunsen's burner.

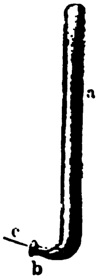

FIG. 68. — Enlarged view of 'wick.' *a*, supply of liquid; *c*, platinum wick.

Blow-pipe flame.

If a higher temperature than that of the Bunsen flame is required, the blow-pipe flame (Fig. 69) may be resorted to; in this the quantity of air and coal-gas or hydrogen is varied at pleasure, and a very high temperature may be obtained.

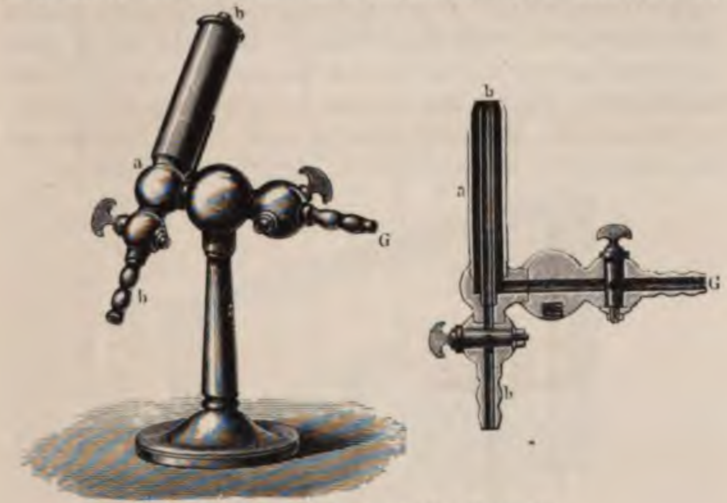

FIG. 69.—Herapath's blow-pipe. View and section.

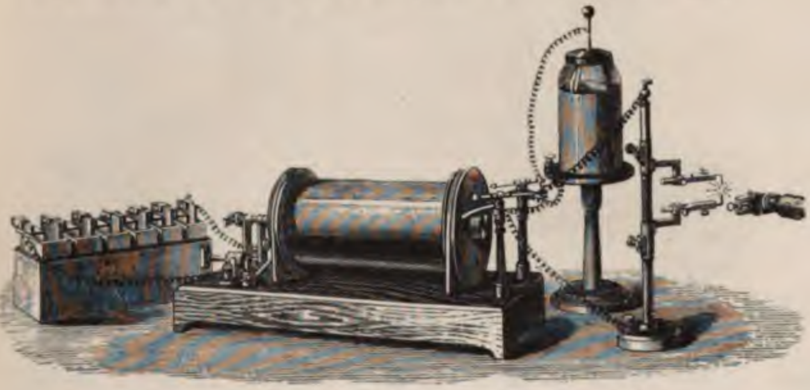

FIG. 70.—Induced current spark.

We might proceed thus to examine the flame spectra of various salts; but to observe the spectra of the vapours

Electric spark.

of the metals, it will be found necessary to use a higher temperature still, and for this purpose the electric arc or spark is employed. If a temperature only slightly greater than that of the blowpipe flame is used, the spark from an induction coil worked by five Grove cells may be taken as shown in Fig. 71, the Leyden jar not being employed; a few metallic lines will then be seen, and a background consisting generally of bands of light here and there.

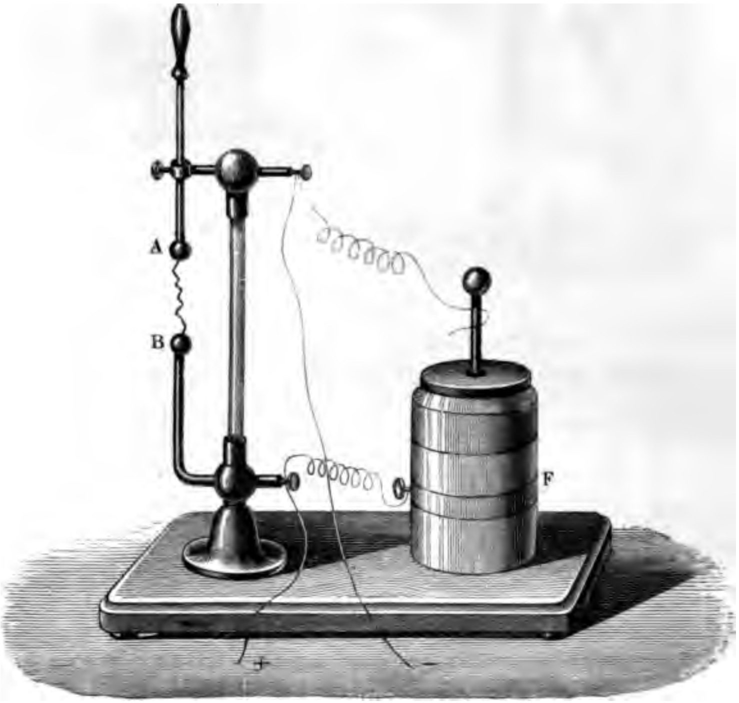

FIG. 71.—Figure showing how the jar is connected with the spark stand. A, B, poles; F, jar.

Leyden jar.

If a higher temperature still is required, the jar must be thrown into the circuit, upon which the spark will become more intense, according to the power of the coil and size of the jar; or the electric arc may be employed.

CHAP. XI.

The spectra thus obtained are much more complex; the spectrum of iron, for instance, when examined at this high temperature, is found to consist of no less than 460 bright lines, many of which are situated in the green part of the spectrum.

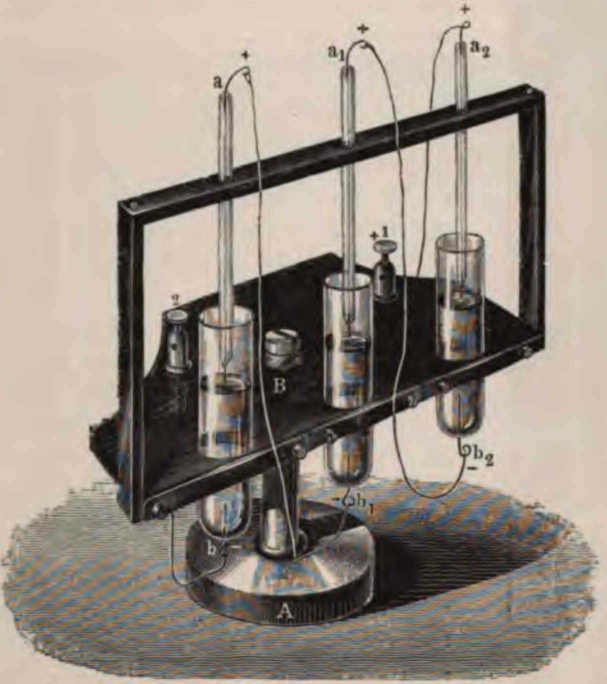

FIG. 72.—Becquerel's arrangement for studying the spark spectra of vapours. *a*, *a* 1, *a* 2, glass tubes with platinum wire fixed in them; *b*, *b* 1, *b* 2. glass vessels with platinum wire fixed into their lower ends. The vessels are partially filled with liquid, and the tubes lowered so that the ends of the platinum wires are near the liquid surface. The spark is then passed, and the spectrum is observed by a spectroscope placed near the vessels.

Spectra of gases.

The manner in which the spectra of vapours may be otherwise obtained will be gathered from Fig. 72. There are also a great many gases which the spectroscopist has to study, and to study with great care. How is this managed? Tubes containing gases are prepared; and when we wish to study the spectrum of a gas, we do it in this way: we enclose it in a tube, and send a current through it by means of an induction-coil. If we pass a stream of electric sparks through a tube containing hydrogen, we shall see that the colour of the incandescent gas is a bright carmine-red, the spectrum of which can easily be observed by placing the spark tube in front of the slit of one of the spectroscopes before described. This arrangement is one that is in daily use in many of our laboratories, and it must be borne in mind as being the *modus operandi* by which a great deal of the work has been done to which I shall have to allude shortly.

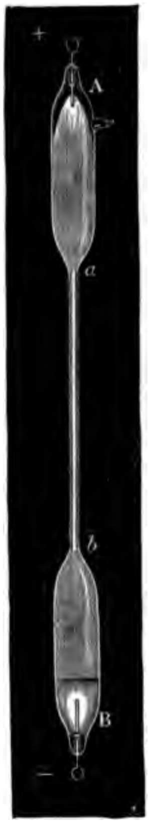

FIG. 73.—Geissler's tube, showing electric discharge.

To sum up, then, in all cases where we are dealing with vapours and not solids or liquids, instead of a continuous spectrum we get a discontinuous one—*i.e.* we get bright lines. This is fact number two.

In the two facts to which I have referred we have, brought sharply to a focus, the labours of a long series of illustrious investigators, from Wollaston, whom, on page 148, we saw observing the spectrum of the electric light to be separated into several images, which he "could not undertake to explain," through Herschel, Fox Talbot, Wheatstone, Masson, Ångström, Plücker, W. A. Miller, Swan, Kirchhoff, and Bunsen.

Spectrum analysis, then, applied to the radiation of light, teaches us that *solids and liquids give out continuous spectra, and that vapours and gases give out discontinuous spectra*; that is to say, that we get bright lines in different parts of the spectrum, instead of having an unbroken light all over the spectrum.

This statement may be varied by stating broadly that the radiation or giving out of light by solids and liquids is a general one, and that the radiation or giving out of light by gases and vapours, instead of being general, is in the main a selective one.

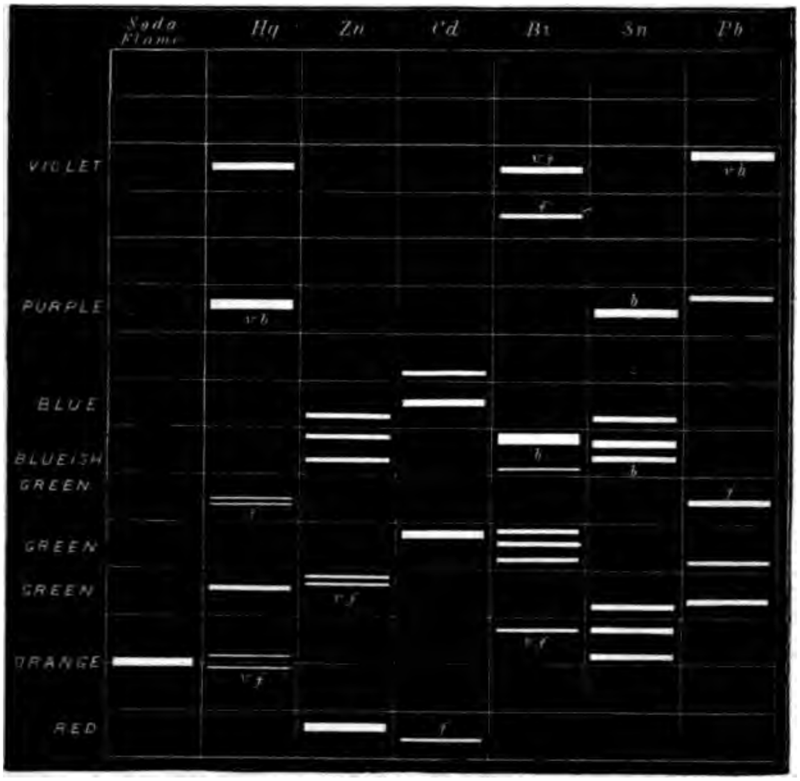

FIG. 74.—Wheatstone's map of the spectra of the vapours of some of the elements (1835).

All discontinuous spectra different.

Further: *the spectrum given out by each element in a state of gas is so different from the spectra of all other elements, that in each case the element to which the spectrum belongs can easily be determined.*

CHAP. XI.

Absorption

We must now pass from the radiation or giving out of light by bodies in different states—that is to say, by solid or liquid bodies, or gaseous or vaporous ones—to the action of the prism upon light under some new conditions. Light is not only given out, or *radiated*, but it may be stopped or *absorbed* in its passage from the light-source to our eye, if we interpose in the path of the beam certain more or less perfectly transparent substances, be they solids, liquids, gases, or vapours. I will recall one or two of the experiments to which reference was made, in order that it may be exactly seen how the perfectly distinct classes of phenomena due to radiation and absorption really run together. It was pointed out that radiation, or the giving out of light, was *continuous* or *selective*.

may be continuous or selective.

Now, radiation is exactly equalled by absorption in this matter; *absorption may also be continuous or selective.* We took before, as an instance of continuous radiation, a continuous spectrum obtained by using the electric lamp or a lime-light: that is to say, an example of the general radiation which we get from an incandescent solid—the carbon points of which the poles of the lamp are composed, or the solid lime. It will also be recollected that when we observed the spectrum of a vapour—as, for instance, that of strontium or thallium—that the continuous spectrum

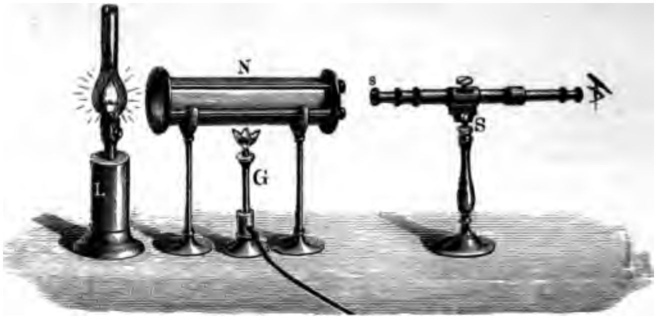

FIG. 75.—Method of observing the absorption of a vapour. L, lamp; S, spectroscope; N, containing tube; G, Bunsen burner.

was altogether changed, and, in place of a beautiful rainbow band, continuous from the red end of the spectrum to the violet, we really only got lines here and there, which are due to selective radiation, and opposed to the general radiation spoken of in the continuous spectrum just now. What I have to dwell on now is, that the absorption or sifting of light by different bodies is very like radiation in its results—that is to say, in some cases we have an absorption which deals equally with every part of the spectrum, and in other cases we have absorption which only picks out particular parts of the spectrum here and there to act upon. But there is one important point to be borne in mind: when dealing with absorption, we must always have a continuous spectrum to act upon. If we had a discontinuous spectrum to act upon, the thing would not be at all so clear. Having this continuous spectrum, the problem is, what the action of the different substances on the light will be. Let me give you an instance of general absorption. If we take the continuous spectrum above referred to, and interpose a piece of smoked glass, or better, a piece of neutral-tint glass, you will find that the substance will cut off the light and deaden the spectrum, so to speak, throughout its whole length. This neutral-tinted glass, then, has the faculty evidently of keeping back the light, red, yellow, blue, green, violet, and so on; and is an instance of general absorption.

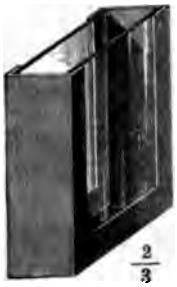

FIG. 76.—Vessel with glass sides for studying the absorption of liquids.

Instances.

A very dense vapour would furnish us with another similar instance. Now if, instead of using neutral-tint glass, a piece of coloured glass is introduced, the action of this, instead of being general throughout the spectrum, will be limited to a particular part of it. Thus, a piece of red glass will cut off nearly all the light except the red: and a piece of blue glass will cut off everything except the blue. By introducing both these pieces in the beam, the spectrum is entirely obliterated.

Analysis by absorption.

In these latter cases we have instances, not of general, but of selective absorption, one substance cutting off everything but the red, and the other cutting off everything but the violet. Now, as different substances are known by their radiation, so also chemists find it perfectly easy to detect different substances by means of their absorption: for instance, the absorption spectrum of nitrous fumes can be shown by taking first our continuous spectrum, which we must always have to start with, and introducing some nitric peroxide between the source of light and the prism. The nitric oxide, immediately it comes in contact with the air, produces dense red fumes, and numbers of fine black lines will be seen immediately crossing the spectrum at right angles to its length, causing it to resemble the

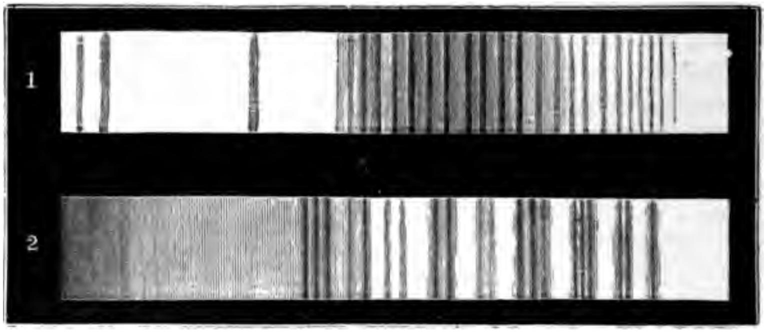

FIG. 77.—Absorption spectra of iodine and nitrous fumes.

solar spectrum with its Fraunhofer lines. Iodine is another substance which gives a coloured vapour, the absorption spectrum of which is very definite and well defined. Fig. 77, Spectrum No. 1, shows the absorption spectrum of iodine vapour, and No. 2 that of nitrous fumes.

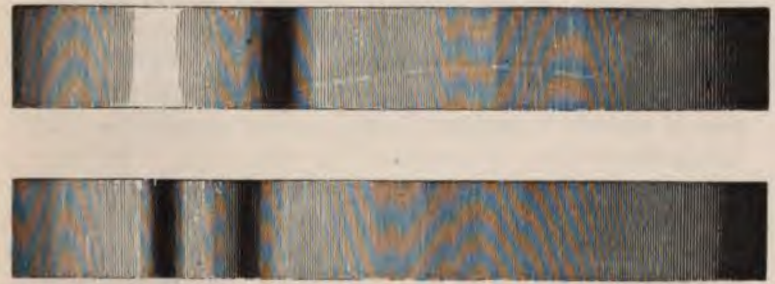

FIG. 78.—Absorption of magenta (above) and blood (below).

We are not limited to these substances ; we may try blood, for instance. We shall find that the action of blood upon the light is perfectly distinct from the action of those fumes which we have spoken of ; and instead of having typical lines in the green and blue parts of the spectrum, we have two very obvious bands in the more luminous part of the spectrum. The colour of a solution of blood is not unlike

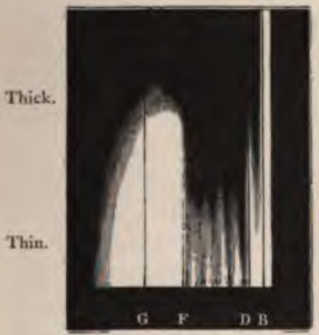

FIG. 79.—Absorption of various thicknesses of a solution of the salts of chromium. (Gladstone.)

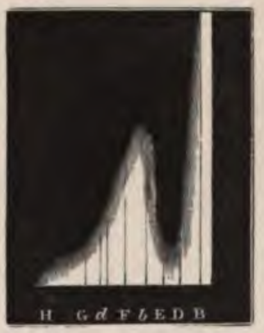

FIG. 80.—Absorption of various thicknesses of a solution of potassic permanganate. (Gladstone.)

the colour of a solution of magenta; but if, instead of using a solution of blood, we use a solution of magenta, we should have only a single black bands. The absorption spectrum of potassic permanganate solution is another beautiful instance. Instead of the two dark band which we saw in the case of the blood, or the single one in the case of magenta, we have four very definite absorption bands in the green part of the spectrum. So that the means of research spectrum analysis affords as far as regards radiation, is entirely reproduced in the case of absorption; and it is perfectly easy, by means of the absorption of different vapours and different substances held in solution, to determine not only what the absorbers really are, but to determine the presence of an extremely small quantity. Further, by allowing the light to pass through a greater thickness of the absorbing substance, the absorption lines are thickened and new regions of absorption are observed. This fact was discovered by Dr. Gladstone, who used hollow prisms containing the substance.

HISTORY OF THE APPLICATION OF THE PRINCIPLES OF SPECTRUM ANALYSIS TO THE SOLAR SPECTRUM.

CHAP. XII.

I HAVE now to attempt to connect the two perfectly distinct classes of phenomena, to which I have referred in the last chapter—the phenomena, namely, of radiation and the phenomena of absorption ; and this connection between radiation and absorption is an instance of the slow growth of science. Fraunhofer, at the beginning of this century, had a very shrewd suspicion of the perfect coincidence of place in the spectrum between certain dark lines which he saw in the spectrum of the sun, as we have seen (p. 149) and the bright lines in the spectrum of a lamp. Fraunhofer at the first suspected, and after him many of our greatest minds suspected, that there was some hidden, wondrously strange, connection between the double yellow line which you will remember is characteristic of sodium, and a certain double line which exists among the black lines of the solar spectrum. Brewster many years worked on the same subject. I have been favoured by Dr. Gladstone with an extract from Dr. Brewster's notebook, dated St. Andrews, October 28th, 1841. In it Brewster says :—" I have this evening discovered the remarkable fact that, in the combustion of nitre upon charcoal, there are definite bright rays corresponding to the double lines of A and B, and the group of lines *a* in the space A B. The coincidence of two yellow rays with the two deficient ones at D, with the existence of

Fraunhofer, 1814.

Brewster, 1841.

definite bright rays in the nitre flame, not only at D but at A, *a* and B, is so extraordinary that it indicates some regular connection between the two classes of phenomena." Let me explain this at once in the light of modern science.

The bright yellow line observed by Fraunhofer and Brewster is the bright line of sodium vapour, and we have established the third fact in spectrum analysis, that the vapours, the radiation of which has been referred to in Chapter XI., absorb, when relatively cool, the same rays which they emit when hot. Hence, the absorption of sodium when cool gives us a dark line in the yellow, as its radiation when hot gives us a bright line in the yellow.

Foucault, 1849.

A discovery made by Foucault in 1849 was the first to suggest this. In describing the result of the prismatic analysis of the Voltaic arc formed between charcoal poles, he remarked:[1]—

"The spectrum is marked, as is known, in its whole extent by a multitude of irregularly grouped luminous lines; but among these may be remarked a double line situated at the boundary of the yellow and orange. As this double line recalled, by its form and situation, the line D of the solar spectrum, I wished to try if it corresponded to it, and, in default of instruments for measuring the angles, I had recourse to a particular process.

"I caused an image of the sun, formed by a converging lens, to fall on the arc itself, which allowed me to observe at the same time the electric and the solar spectrum superposed; I convinced myself in this way that the double bright line of the arc coincides exactly with the double dark line of the solar spectrum.

"This process of investigation furnished me matter for some unexpected observations. It proved to me, in the first instance, the extreme transparency of the arc, which occasions only a faint shadow in the solar light. It showed me that this arc, placed in the path of a beam of solar light, absorbs the rays D, so that the above-mentioned line D of the solar light is considerably strengthened when the two spectra are exactly superposed. When, on the contrary, they jut out one beyond the other, the line D appears darker than usual in the solar light, and stands out bright in the electric spectrum, which allows one easily to judge of their perfect coincidence. Thus the arc presents us with a medium which emits the rays D on its own account, and which at the same time absorbs them when they come from another quarter.

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[1] *L'Institut*, Feb. 7, 1849, translated by Prof. Stokes, in *Phil. Mag.* vol. xix. p. 194.

"To make the experiment in a manner still more decisive, I projected on the arc the reflected image of one of the charcoal points, which, like all solid bodies in ignition, gives no lines; and under these circumstances the line D appeared to me as in the solar spectrum."

Stokes, 1852.

The explanation of this coincidence between the two bright lines of burning sodium vapour, and the two dark lines D in the solar spectrum, which extended the grasp of spectrum analysis from terrestrial substances to the skies, was first given by Professor Stokes about 1852.

The observational and experimental foundation on which Stokes based his explanation, has thus been stated by Sir William Thomson:[1]—

The facts before him.

"1. The discovery by Fraunhofer of a coincidence between his double dark line D of the solar spectrum and a double bright line which he observed in the spectra of ordinary artificial flames.

"2. A very rigorous experimental test of this coincidence by Prof. W. H. Miller, which showed it to be accurate to an astonishing degree of minuteness.

"3. The fact that the yellow light given out when salt is thrown into burning spirits consists almost solely of the two nearly identical qualities which constitute that double bright line.

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[1] President's Address, British Association Meeting, 1871.

"4. Observations made by Stokes himself, which showed the bright line D to be absent in a candle-flame when the wick was snuffed clean, so as not to project into the luminous envelope, and from an alcohol flame when the spirit was burned in a watch-glass. And

"5. Foucault's admirable discovery, already referred to, that the Voltaic arc between charcoal points is 'a medium which emits the rays D on its own account, and at the same time absorbs them when they come from another quarter.'"

His conclusions.

The conclusions, theoretical and practical, which Stokes taught to Sir William Thomson, and which the latter gave regularly afterwards in his public lectures in the University of Glasgow, were:

"1. That the double line D, whether bright or dark, is due to vapour of sodium.

"2. That the ultimate atom of sodium is susceptible of regular elastic vibrations, like those of a tuning-fork or of stringed musical instruments; that like an instrument with two strings tuned to approximate unison, or an approximately circular elastic disc, it has two fundamental notes or vibrations of approximately equal pitch; and that the periods of these vibrations are precisely the periods of the two slightly different yellow lights constituting the double bright line D.

"3. That when vapour of sodium is at a high enough temperature to become itself a source of light, each atom executes these two fundamental vibrations simultaneously; and that therefore the light proceeding from it is of the two qualities constituting the double bright line D.

"4. That when vapour of sodium is present in space across which light from another source is propagated, its atoms, according to a well-known general principle of dynamics, are set to vibrate in either or both of those fundamental modes, if some of the incident light is of one or other of their periods, or some of one and some of the other; so that the energy of the waves of those particular qualities of light is converted into thermal vibrations of the medium and dispersed in all directions, while light of all other qualities, even though very nearly agreeing with them, is transmitted with comparatively no loss.

"5. That Fraunhofer's double dark line D of solar and stellar spectra is due to the presence of vapour of sodium in atmospheres surrounding the sun and those stars in whose spectra it had been observed.

"6. That other vapours than sodium are to be found in the atmospheres of sun and stars by searching for substances producing in the spectra of artificial flames bright lines coinciding with other dark lines of the solar and stellar spectra than the Fraunhofer line D."

Explanation.

The idea then of Stokes which connected radiation with absorption, and at once read the riddle set by the sun and stars, was this: the light emitted by an incandescent vapour is due to the vibrations of its molecules, as a sound-note emitted by a piano-wire is due to the vibration of the wire. We have only to go into a room where there is a piano, and sing a note, to find that the wire which corresponds to our note will respond to it. Now, in the same way when light is passing through a vapour the molecules of which vibrate at any particular rate, they will be urged into their own special rate of vibration by the vibrations of the light which is passing through them, which correspond to that particular rate. Hence the light will, so to speak, be sifted, and the force it has exercised in impelling the particles in the interrupting vapour to vibrate will tell upon it; and in this way, those particular vibrations which have had the work to do will be enfeebled.

It is clear that the parts of the spectrum thus reduced in brilliancy will depend upon the vapour through which the light has passed. If sodium vapour be traversed, then the light corresponding to the bright lines of sodium will be enfeebled.

Stokes' statement.

Stokes first gave this dynamical illustration in the following words:[1] "That a body may be at the same time a source of light giving out rays of a definite refrangibility, and an absorbing medium extinguishing rays of that same refrangibility which traverse it, seems readily to admit of a dynamical illustration borrowed from sound. We know that a stretched string which on being struck gives out a certain note (suppose its fundamental note), is capable of being thrown into the same states of vibration by aërial vibrations corresponding to the same note. Suppose now a portion of space to contain a great number of such stretched strings, forming thus the analogue of a 'medium.' It is evident that such a medium on being agitated would give out the note above mentioned; while, on the other hand, if that note were sounded in air at a distance, the incident vibrations would throw the strings into vibration, and consequently would themselves be gradually extinguished, since otherwise there would be a creation of *vis viva*. The optical application of this illustration is too obvious to need comment."

Such was the theory which as I have shown was taught by Stokes prior to 1852, and by Thomson in his public lectures ever since.

Our great physicist unfortunately did not publish it. I say unfortunately because valuable time has been lost: but not much, so far as publication or non-publication is concerned; for, in 1853, the idea was published by the celebrated Ångström.[2]

Ångström, 1853.

In his memoir, for the purpose of illustrating the absorption of light, he made use of a principle already propounded by Euler, in his *Theoria lucis et caloris*, that the particles of a body, *in consequence of resonance*, absorb principally those ethereal undulatory motions which have previously been impressed upon them. He also endeavoured to show that *a body in a state of glowing heat emits just the same kinds of light and heat which it absorbs under the same circumstances.* He further undertook researches on the electric light, and stated that in many cases the Fraunhofer lines were an inversion of the bright lines, which he observed in the spectrum of various metals.[1]

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[1] Phil. Mag. vol. xix. p. 196.

[2] "Optiska Undersokningar:" Trans. Royal Academy of Stockholm, 1853. Translated in *Phil. Mag.* 4th Series, vol. ix. p. 237.

Stewart, 1859.

Early in 1859, Mr. Balfour Stewart independently discovered the law which binds together radiation and absorption, establishing it experimentally as an extension of Prévost's law of exchanges in the case of the heat rays, and generalizing his conclusion for all rays.[2] Stewart's reasoning is of a very ingenious and original nature.

In October of the same year, 1859, Kirchhoff established experimentally the same law for the light rays.

Kirchhoff, 1859.

His first announcement, dated Heidelberg, 20th October, 1859, read before the Berlin Academy on the 27th,[3] must here be given *in extenso,* for it has certainly marked an epoch in solar physics.

"On the occasion of an examination of the spectra of coloured flames not yet published, conducted by Bunsen and myself in common, by which it has become possible for us to recognize the qualitative composition of complicated mixtures from the appearance of the spectrum of their blow-pipe flame, I made some observations which disclose an unexpected explanation of the origin of Fraunhofer's lines, and authorize conclusions therefrom respecting the material constitution of the atmosphere of the sun, and perhaps also of the brighter fixed stars.

"Fraunhofer had remarked that in the spectrum of the flame of a candle there appear two bright lines which coincide with the two dark lines D of the solar spectrum. The same bright lines are obtained of greater intensity from a flame into which some common salt is put. I formed a solar spectrum by projection, and allowed the solar rays concerned, before they fell on the slit, to pass through a powerful salt flame. If the sunlight were sufficiently reduced, there appeared in place of the two dark lines D two bright lines; if, on the other hand, its intensity surpassed a certain limit, the two dark lines D showed themselves in much greater distinctness than without the employment of the salt flame.

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[1] See *Phil. Mag.* 4th Series, vol. xxiv. pp. 2, 3; *Monatsbericht,* 1859, p. 662.

[2] Edinburgh Transactions, 1858-9.

[3] See Translation by Professor Stokes in *Phil. Mag.* 4th Series, vol. xix. p. 195.

"The spectrum of the Drummond light contains, as a general rule, the two bright lines of sodium, if the luminous spot of the cylinder of lime has not long been exposed to the white heat; if the cylinder remains unmoved, these lines become weaker, and finally vanish altogether. If they have vanished, or only faintly appear, an alcohol flame into which salt has been put, and which is placed between the cylinder of lime and the slit, causes two dark lines of remarkable sharpness and fineness, which in that respect agree with the lines D of the solar spectrum, to show themselves in their stead. Thus the lines D of the solar spectrum are artificially evoked in a spectrum in which naturally they are not present.

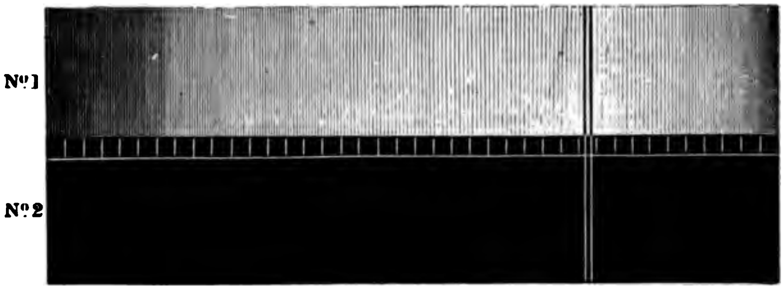

FIG. 81.—Coincidence between the bright line given out by sodium vapour and the dark line produced by the absorption of the light of a continuous spectrum by its passage through sodium vapour.

"If chloride of lithium is brought into the flame of Bunsen's gas-lamp, the spectrum of the flame shows a very bright sharply defined line, which lies midway between

Effect of brighter light source.

Fraunhofer's lines B and C. If, now, solar rays of moderate intensity are allowed to fall through the flame on the slit, the line at the place pointed out is seen bright on a darker ground; but with greater strength of sunlight there appears in its place a dark line, which has quite the same character as Fraunhofer's lines. If the flame be taken away, the line disappears, as far as I have been able to see, completely.

Solar lines produced by certain known substances.

"I conclude from these observations, that coloured flames in the spectra of which bright sharp lines present themselves, so weaken rays of the colour of these lines, when such rays pass through the flames, that in place of the bright lines dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in the spectrum of which these lines are otherwise wanting. *I conclude further, that the dark lines of the solar spectrum which are not evoked by the atmosphere of the earth exist in consequence of the presence, in the incandescent atmosphere of the sun, of those substances which in the spectrum of a flame produce bright lines at the same place.* We may assume that the bright lines agreeing with D in the spectrum of a flame always arise from sodium contained in it; the dark line D in the solar spectrum allows us, therefore, to conclude that there exists sodium in the sun's atmosphere. . . . . .

"In the course of the experiments which have at present been instituted by us . . . . . a fact has already shown itself which seems to us to be of great importance. The Drummond light requires, in order that the lines D should come out in it dark, a salt-flame of lower temperature. The flame of alcohol containing water is fitted for this, but the flame of Bunsen's gas-lamp is not. With the latter the smallest mixture of common salt, as soon as it makes itself generally perceptible, causes the bright lines of sodium to show themselves. . . . . ."

Immediately after the publication of this important note of Kirchhoff's, Stewart[1] explained, in extension of his former work on the theory of exchanges, why it was that a salt flame of lower temperature was required to darken the D lines, pointing out that it was a phenomenon analogous to that presented when a piece of ruby glass is heated in the fire. So long as the ruby glass is cooler than the coals behind it, the light given out is red because the ruby glass stops the green, the green light is therefore analogous to the line D which is given out by an alcohol flame into which salt has been put. Should however this ruby glass be of a much higher temperature than the coals behind it, the greenish light which it radiates overpowers the red which it transmits, so that the light which reaches the eye is more green than red. This is precisely analogous to what is observed when a Bunsen's gas flame with a little salt is placed in front of the Drummond light, when the line D is no longer dark but bright.

Ruby glass experiment.

Shortly afterwards Kirchhoff independently explained his results on the same theory.

In the next chapter we shall show how fruitful of result this experimental verification of Stokes' theory by Kirchhoff and Stewart, really effected by Foucault, has been, and how the new field of investigation thus opened up has been explored.

Here we may content ourselves by pointing out how, by the light of modern science, a simple experiment, made by means of sodium vapour and a beam of sunlight, with the powerful aid of a little prism, gave us a tremendous increase of our knowledge about distant worlds—worlds so immeasurably remote that it seemed hopeless for men to try and grapple with them.

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[1] See "On the Theory of Exchanges and its recent Extension," by Balfour Stewart, B.A., Reports, 1861. I quote here the passage which relates to the connection between the heat and light rays :—

"We come now to the subject of light ; and since radiant light and heat have been shown by Melloni, Forbes, and others, to possess very many properties in common, it was of course only natural to suppose that facts analogous to those mentioned should hold also with regard to light. One instance will at once occur, in which this analogy is perfect. For, as all opaque bodies heated up to the same temperature radiate the same description of heat, so also when their common temperature is still further increased, they acquire a red heat, or a yellow heat, or a white heat, simultaneously.

"The idea of applying these views to light had occurred independently to Professor Kirchhoff and myself; but, although Kirchhoff slightly preceded me in publication, it will be convenient to defer the mention of his researches till I come to the subject of lines in the spectrum.

"In February 1860, I communicated to the Royal Society of London a paper in which certain properties of radiant light were investigated, similar to those already treated of with respect to heat. In this paper it was mentioned that the amount of light radiated by coloured glasses is in proportion to their depth of colour, transparent glass giving out very little light; also that the radiation from red glass has a greenish tint, while that from green glass has a reddish tint.

"It was also mentioned that polished metal gives out less light than tarnished metal, and that, when a piece of black and white porcelain is heated in the fire, the black parts give out much more light than the white, thereby producing a curious reversal of the pattern.

"All these facts are comprehended in the statement that in a constant temperature the absorption of a particle is equal to its radiation, and that for every description of light.

"It was also noticed that all coloured glasses ultimately lose their colour in the fire, as they approach in temperature the coals around them; the explanation being, that while red glass, for instance, gives out a greenish light, it passes red light from the coals behind it, while it absorbs the green, in such a manner that the light which it radiates precisely makes up for that which it absorbs, so that we have virtually a coal radiation coming partly from, and partly through, the glass.

"In another paper, communicated to the Royal Society in May of the same year, it is shown that tourmaline, which absorbs in excess the ordinary rays of light, also radiates, when heated, this description of light in excess, but that when the heated tourmaline is viewed against an illuminated background of the same temperature as itself, this peculiarity disappears."

RESULTS OBTAINED BY THE OLD METHOD.

I. KIRCHHOFF'S MAPS AND LIST OF ELEMENTS IN THE SUN.

Sodium.

IN the note of Kirchhoff's which was given almost *in extenso* in the last chapter, we saw the demonstration of the fact of the existence of sodium in a relatively cool atmosphere surrounding the sun. Now sodium has a very simple spectrum; Kirchhoff was not long before he tested his generalization by the most complicated spectrum he could find. He took for this purpose the spectrum of iron, one of enormous complication, for, as we now know, the spectrum is traversed by lines throughout its whole length, no less than 460 lines having been already mapped, and their positions are now thoroughly well known to us—as well known as the position of any star in the heavens. Kirchhoff tried the iron spectrum, and he found, absolutely corresponding in position and in width and darkness to the bright iron lines which he saw in this spectrum, black lines in the solar spectrum. He instantly convinced himself and soon convinced the world, that he had experimentally established not only the fundamental fact, which we have already stated, that gases and vapours have the power of absorbing those very rays which they themselves give out when in a state of incandescence, but that iron as well as sodium was present in the sun. Kirchhoff went on with this magnificent work—which included the construction of the first map and catalogue of the

Iron.

lines of the solar spectrum approaching completeness, which had ever been given to the world ; a monument of industry which cost the illustrious physicist one of his eyes — until he had arrived at the conclusion that sodium, iron, calcium, magnesium, nickel, barium, copper, zinc, [potassium ?] were in the solar atmosphere ; that the existence of cobalt there was doubtful, and that gold, silver, mercury, aluminium, cadmium, tin, lead, antimony, arsenic, strontium, lithium, and silicon, were absent.

Calcium, magnesium, barium, copper, and zinc, added to the list.

II. Ångström's Map and List.

Kirchhoff, however, was not the only one at work at the problem. I have already alluded to Ångström. He, too, like Kirchhoff, was constructing a map of the lines in the solar spectrum, with the important difference that whereas Kirchhoff's scale was an arbitrary one, Ångström's scale was based upon the lengths of the light waves. Nor was this all,

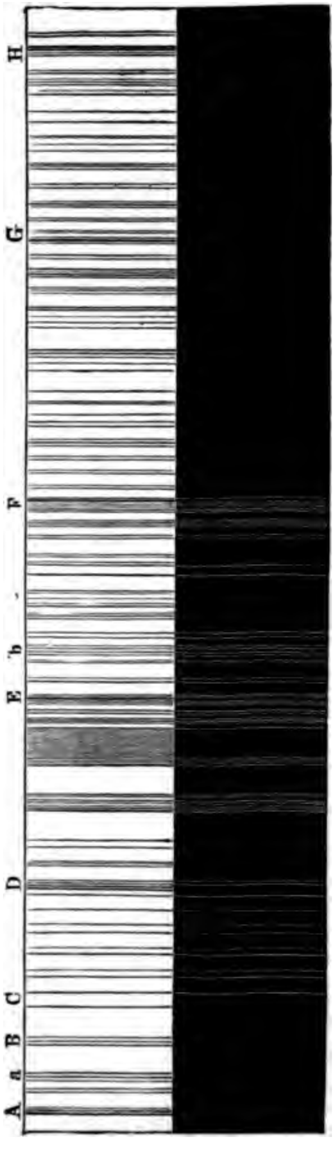

FIG. 82.—Coincidence of some of the bright lines of iron with some of the Fraunhofer lines.

he also set himself to determine the wave lengths of the bright lines given out by the various gases and vapours in the electric arc.

This he accomplished by obtaining a spectrum by diffraction instead of by refraction. The way in which a spectrum is obtained by refraction has been already explained. In diffraction observations a grating is used, placed usually at right angles to the axis of the collimator; this replaces the prism used when refraction is employed: the grating consisting of excessively fine parallel equidistant lines scratched on glass by means of a diamond and dividing engine; in some cases 4000 or 5000 or even 6000 lines are thus produced in an inch.

The diffraction spectrum.

The diffraction spectrum results from interference, and the grating was suggested by Fraunhofer, who made the first observations of this nature by using fine wires. Its use was suggested by the fact that the two mirrors or the biprism ordinarily employed may be replaced by two narrow apertures.

The wave length is determined by accurate measures of the angle between the observing telescope and the axis of the collimator, and of the distance between the scratches, the wave length being included in the formula[1]—

$$\sin \delta = n \frac{\lambda}{d}$$

Where δ = observed angle,
n = order of the spectrum,
d = distance between lines in millimetres,
λ = wave length.

The wave lengths of the principal Fraunhofer lines thus determined are laid down in Ångström's map of the solar spectrum,[2] on a scale such that the wave length can be read off to a ten millionth of a millimetre (0.0000001), and estimated to a hundred millionth (0.00000001). The wave lengths of some of the principal Fraunhofer lines are as follows:—

---

[1] For further details see "Jamin's *Cours de Physique*," t. iii., p. 530, *et seq.*

[2] "Recherches sur le Spectre Solaire," par R. J. Ångström. Spectre normal du Soleil. Atlas de six planches. Upsala: W. Schultz. Berlin: F. Dümmler, 1869.

A	0·00076009	E	0·00052689
B	0·00068668	F	0·00048606
C	0·00065618	G	0·00043072
D′	0·00058950	H′	0·00039680
D″	0·00058890	H″	0·00039328

It will be seen from the above table that the wave length decreases from the red end of the spectrum to the violet; in other words that the red waves are the longest and the violet waves the shortest in the spectrum; and as the velocity of light is 298,000,000 metres, or 298,000,000,000, millimetres, or 29,800,000,000,000,000,000 hundred-millionths of a millimetre per second, it is clear that the number of waves (which number will vary with each wave) which fall on the eye per second can be determined. Thus we have for A $\frac{29{,}800{,}000{,}000{,}000{,}000{,}000}{76009} = 392$ billions per second (roughly) For H_2 $\frac{29{,}800{,}000{,}000{,}000{,}000{,}000}{39328}$ = 757 billions per second (roughly).

No. of waves per second of time of A and of H_2.

As the velocity of light is the same for ali waves, it follows that the number of waves per second varies inversely as the wave length in each case, and that the number of each wave per second multiplied into its wave length must give us a constant quantity, namely the velocity.

The first results[1] of Ångström's comparison of the wave-lengths of metallic vapours with the Fraunhofer lines added the possibility of strontium and aluminium being among the solar elements, and this observer thus allocated the principal Fraunhofer lines.

H^1 and H^2 due to Calcium.
G „ Iron.
F „ Strontium and Iron (uncertain).

---

[1] Communicated to the Royal Academy of Stockholm, October 8, 1861. *Phil. Mag.*, S. 4, vol. xxiv., p. 1.

b	due to	Magnesium and Iron.
D	„	Sodium.
C	„	Hydrogen.
B	„	Potassium.

Thalen rejects zinc and aluminium, and adds manganese and titanium to the solar elements.

Ångström later, in his *Recherches sur le Spectre Solaire,* gives more details, and his assistant Thalen[1] sums up the work at that time by rejecting zinc and aluminium from the list of solar elements as given by Kirchhoff, and adding manganese and titanium.

III. FIZEAU'S SUGGESTION AS TO EFFECT OF MOTION ON WAVE LENGTH.

There is a point of fundamental importance to solar physics connected with this method of wave length measurement: not only does it give us a spectrum in which the position of each line gives us its wave-length, but it also gives us a power of detecting motion of the vapours in the solar atmosphere if such exist, according to a principle first indicated by Fizeau, that if a light source moves to or from the eye with a velocity comparable with the velocity of light itself, then a change in the position of the lines in the spectrum will be brought about.

Motion of the light source attended by displacement of the spectral line due to it.

M. Fizeau's researches in this direction are so little known in this country, that I give the account of them at length.[2]

> "Si un corps sonore émettant un son continu et toujours identique se meut avec une vitesse comparable à celle du son, les ondes sonores ne seront pas symétriquement disposées autour du corps sonore, comme cela à lieu lorsqu'il est au repos ; mais elles seront plus rapprochées les unes des autres dans la région vers laquelle aura lieu le mouvement, et plus éloignées dans la région opposée, pour un observateur placé en avant ou en arrière du corps sonore ; le son sera donc différent ; plus aigu dans la première position, plus grave dans la seconde.
>
> "Si l'observateur à son tour est supposé en mouvement, le corps sonore restant immobile, le résultat sera semblable, mais la loi du phenomène est différente.

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[1] "Longueurs d'onde des raies métalliques." Nova Acta. Upsala.

[2] Société Philomathique de Paris. Extraits des proces-verbaux des Sciences. 23 Dec., 1848. p. 81.

"En calculant les vitesses qui correspondent aux intervalles de la gamme on trouve les nombres suivants : pour produire une élévation d'un *demi-ton*, le corps sonore doit avoir une vitesse par seconde de 21·25, pour un ton majeur 37·8, pour la tierce 68, pour l'octave 170. Dans le cas du corps sonore immobile et pour obtenir les mêmes notes l'observateur doit avoir les vitesses, 22·6 : 42·5 : 85 ; et 340. Les sons émis ou reçus, dans des directions différentes de celles du mouvement se calculent en projetant la vitesse sur la nouvelle direction.

L'auteur donne la description d'un appareil qu'il a employé et au moyen duquel on peut vérifier et démontrer commodément ces curieuses propriétés du son, dans le cas du mouvement du corps sonore. Cette appareil est fondé sur le principe des roues dentées de M. Savart, mais la disposition est inverse. Au lieu de dents mobiles rencontrant dans leur mouvement un corps élastique fixe, c'est le corps élastique qui est placé sur la circonférence d'une roue et qui recontre dans son mouvement des dents fixes placées sur la concavité d'un arc extérieur immobile, l'on a ainsi un appareil fixe qui jouit de la propriété d'emettre des sons différents dans chaque direction particulière. Pour une certaine vitesse de rotation par example on aura en avant le son fondamental, en arrière l'octave, et toutes les notes de la gamme dans des directions intermédiaires.

"En appliquant ces considérations à la lumière on arrive à des conséquences curieuses et qui pourraient acquerir de l'importance si l'experience venait à les confirmer. Un mouvement très rapide et comparable à la vitesse de la lumière, attribué au corps lumineux ou à l'observateur, aura pour effet d'altérer la longueur d'ondulation de tous les rayons simples qui composent la lumière reçue dans la direction du mouvement. Cette longueur sera augmentée ou diminuée suivant le sens du mouvement. Considéré dans le spectre, cet effet se traduira par un *déplacement des raies* correspondant au changement de la longueur d'ondulation.

"En calculant la valeur du déplacement angulaire de la raie D dans le cas ou le corps lumineux aurait la vitesse de la planète Vénus, le spectre étant formé au moyen d'un prisme de flint de 60° on trouve 2″, 65.

"Pour le cas où l'observateur seul serait en mouvement et animé d'une vitesse égale à celle de la Terre, on trouve 2″, 25.

"En supposant que l'on mesure les déviations doubles et que l'on se place successivement dans des conditions où les mouvements en question seraient de signe contraire, ces quantites peuvent être quadruplées, et l'on a 10″, 6 et 9 pour les valeurs précédentes.

"L'auteur termine en examinant si ces conséquences pourront être soumises à l'observation, et il pense que les difficultés ne sont pas telles qu'on ne puisse esperer de les surmonter."

To see the bearing of this let us suppose that one of the constituents of the solar atmosphere, let us say hydrogen, while it was giving out its light was moving towards us at the rate of say 50 miles (= 80466 metres) a second. Now the wave length of one of the lines due to hydrogen—

F is, as we have seen, 0·00048606 at the normal velocity. Now with a higher velocity the number of crests per second reaching the eye must be greater, and therefore ***the effective wave length must be shorter.*** In the spectrum this shortening of the wave is indicated by the position of the line F changing towards the violet—the region of shorter waves.

If we supposed the hydrogen receding from the eye, then the position of the line would be changed towards the region of longer waves—***i e.*** towards the red.

Waves lengthened by recession.

Let us suppose such a change to be observed. Suppose a change into a less refrangible region, say a change of the F line, the normal wave length of which is 0·00048606 from that position to 0·00048716. Obviously the wave has been lengthened by the ***recession*** of the source of light from the eye, and the amount of recession, about 39 miles a second, is measured by the increased length of wave, the difference in the wave length bearing the same ratio to the total wave length as the difference in the velocity bears to the velocity of light.[1] Similarly if we suppose a change to a more refrangible region, the wave has been shortened by the approach of the light source.

Shortened by approach.

IV. THE TELLURIC LINES.

The Telluric lines.

At the same time that many of the lines in the solar spectrum were being shown to be due to the presence of vapours in the sun, others were found to be due to absorption by the atmosphere of the earth, a question first investigated by Brewster, later by Brewster and Gladstone, and afterwards by Piazzi Smyth, who gave much attention to the problem in his famous "Astronomer's Experiment" on the Peak of Teneriffe. This fact was first suspected because certain lines were found to vary with the height of the sun. They were very faint when the sun was high, and got thicker and darker as the sun approached the horizon.

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[1] See Clerk Maxwell, *Phil. Trans.* 1868, p. 532.

Janssen's experiment on the absorption of aqueous vapour.

To Janssen belongs the credit of determining to what constituent of our atmosphere this absorption is due. In 1864 this eminent physicist was sent on a mission by the enlightened authorities of the French Academy of Sciences, to investigate this problem, and among other results he observed the spectrum of a large bonfire through a thickness of atmosphere of 21,000 metres over the Lake of Geneva. In this experiment he saw the well-known lines; though close to the fire there was no absorption whatever. On his return to Paris he was enabled to show by the following beautiful experiment that the lines are due to the *aqueous vapour* in our atmosphere. He used a long iron cylinder (in fact a long length of gas-main, which the Paris Gas Company had placed at his disposal) and filled it with steam, taking precautions to keep the temperature high, and the glass ends transparent. At one end of this he placed a bright flame, at the other he observed the light by means of a spectroscope after it had traversed the whole length of tube. He thus obtained a spectrum which was the exact equivalent of that which is superadded on the true solar one, and which becomes

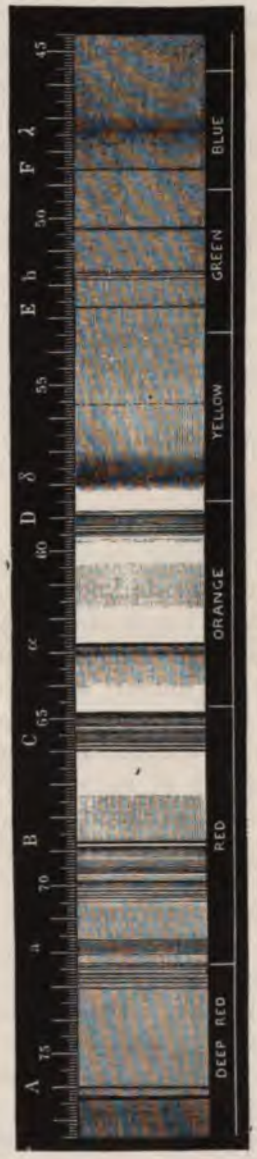

FIG. 83.—The Telluric lines.

most marked when, the sun being low, there is the greatest possible thickness of our atmosphere and its contained aqueous vapour to give rise to a large amount of absorption.

Brewster, Brewster and Gladstone, Piazzi Smyth, Janssen and Ångström have all mapped this spectrum; the most elaborate map we have, a reduced copy of which is given in the accompanying woodcut, is due to the latter.

It follows, from an inspection of Ångström's larger map, that nearly all the lines in the spectrum less refrangible than C are due to absorption by the aqueous vapour of our planet.

V. KIRCHHOFF AND ÅNGSTRÖM'S CONCLUSIONS AS TO THE CONSTITUTION OF THE SUN.

By the investigations into the actual presence of elements in the solar atmosphere to which we have before alluded, it was made known to us that in a relatively cool atmosphere surrounding the sun, the elements named were in a state of vapour. The riddle of the sun was read to a certain extent, and Kirchhoff read it in this way. He said:—"There is a solid or a liquid something in the sun, giving a continuous spectrum, and around this there are vapours of sodium, of iron, of calcium, of chromium, of barium, of magnesium, of nickel, of copper, of strontium, of cobalt, and of aluminium; all these are existing in an atmosphere and are stopping out the sun's light. If the sun were not there, and if these things were observed in an incandescent state, we should get exactly these bright lines from them."

Kirchhoff holds that there is a solid or liquid nucleus in the sun giving a continuous spectrum.

Kirchhoff further imagined that the visible sun, the sun which we see—and we may take the sun as an example of every star in the heavens—was *liquid*.

Now in the sun we have, first, a shining orb, dimmed to a certain degree at the edge, and here and there, over the sun, we see what are called spots. Kirchhoff wished, not only to connect his discoveries with the solar atmosphere, but was anxious to connect it with this dimming near the limb and the spots. He said that the solar atmosphere, to which all the absorption lines were due, was really outside the sun, and formed the corona; and that the dimming of the limb was due to the greater absorption of this atmosphere, owing, of course, to the light of the sun travelling through a much greater thickness of atmosphere in reaching our eye from the limb than from the centre of the disc. Furthermore, he said that the sun-spots, which astronomers, from the time of Wilson, had asserted to be cavities, were nothing but clouds floating in this atmosphere of vapour.

Surrounded by vapour producing absorption.

And that the spots are clouds.

Such, then, is Kirchhoff's theory of the sun. There is a something—Kirchhoff said it was a liquid—which gives us a continuous spectrum, and between our eye and that incandescent liquid surface there is an enormous atmosphere, built up of vapours of sodium, iron, and so on; and the reason that we get the dark lines is, that the molecules of the substances named absorb certain rays, those, namely, which they produce when they are in an incandescent state.

Ångström agrees with Kirchhoff.

Ångström on his part arrived at the same, or nearly the same, conclusion. He imagined that the Fraunhofer lines originate for the most part in the photosphere, or the gaseous envelope which immediately surrounds the sun. On this he makes the following important remark:—

"It has been urged as an objection to the hypothesis that the Fraunhofer lines belong principally to the sun, that they ought in that case to appear stronger and more distinct when the rays come from the edge, than when they come from the centre of the disc, which according to Forbes's observation, was not the case. Forbes's observation took place under the different stages of a solar eclipse, under which circumstances I conceive it would be very difficult to preserve the appearance of the spectrum accurately in the memory. I therefore considered that experiment to be worth repeating. I used for this purpose an optical

He experiments on the different absorptions of the disc, and the limb of the sun.

theodolite with two telescopes, one of which was furnished with a slit opening to admit the sunlight; the height of the orifice was reduced to about 4 millims, and the sun image was projected upon it from a Dollond object-glass, of 3·02 meters focal distance. The diameter of the image thus formed was 128 millims, and by allowing the rays from different parts of this image to fall successively upon the opening, it was easy to see whether the Fraunhofer lines underwent any change. Any very remarkable change I could not discover. All that I fancied I could remark was that the intensity of the spectrum light is somewhat less when the ray comes from the edge than when from the centre of the disc; and this is evidenced by the circumstance that the fainter Fraunhofer lines show themselves in the latter case comparatively stronger, whereas, when the light comes from the centre of the solar disc, the fainter lines will sometimes even totally disappear, while the stronger lines, as for example some of the iron lines, appear with correspondingly increased brilliancy; as we know by Kirchhoff's experiments that an increased difference of intensity between the source of light and the absorbing gas is favourable to the distinctness of the lines in the gas spectrum, it would seem that this observation is not repugnant to what we already know concerning the absorbing power of gases."

VI. STONEY'S CONCLUSIONS.

Such then was the theory of the physical constitution of the sun propounded by Kirchhoff and Ångström, as a result of their spectroscopic enquiries. I have now to refer to still another, based upon an examination of the solar spectrum which we owe to Mr. Stoney, a theory which will impress all who read it with its masterly treatment of a confessedly difficult subject.[1]

Mr. Stoney, far from agreeing with Kirchhoff that the spots are clouds, refers them like Wilson to a dark body within a concentric luminous film at a vast depth beneath the surface of the sun's atmosphere, the part of that atmosphere above the photosphere being "enormously transparent to most of the rays which emanate from the shell of clouds." But Mr. Stoney does not stop here, he goes on to add that "the upper layers of the atmospheres of sodium, magnesium and hydrogen, are cooler than those of iron and calcium, that these again are cooler than the upper layers of the atmospheres of nickel, cobalt, copper and zinc;" thus "we have evidence both that the atmospheres of the several gases extend to different heights, and that the temperature increases from the surface of the solar atmosphere downwards. He next suggests that the probable order of the elements in the atmosphere beginning at the outer boundary depends upon their atomic weights. In this way we should have the atmosphere of hydrogen far overlapping all the rest; then, at a profound depth, sodium and magnesium reaching nearly to the same height, as the masses of their molecules are nearly equal; next, at a great distance further down, calcium; then, in a group reaching nearly to the same height, chromium, manganese, iron, nickel and cobalt; then, within a moderate distance of these, copper and zinc; and lastly, after a vast interval, barium.

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[1] *Proc. R.S.* vol. xvii. p. 1, *et seq.*

Theory of shells.

This theory led Mr. Stoney before the eclipse of 1868 to predict, with a considerable approach to truth, some of the results which have been since obtained.

"When examined through a spectroscope, adapted to an equatorial telescope, much of the light of the corona may be found to resolve itself into a multitude of bright lines, the brightest being coincident with the faintest of Fraunhofer's lines. If this should prove to be the case, and if the observer could train himself to distinguish in the hurry and under such novel circumstance the lines of the different gases, it would even be possible to ascertain how high in the sun's atmosphere each reaches, by using a curved slit, and noting the moment at which each set of lines is obliterated by the advancing moon. This would be a determination of exceeding interest. The observations should commence immediately after the beginning of totality, and be kept up to the end of it, as it is only from situations close to the sun's disc that the brightest lines can come.

"Directly outside the photosphere there lies a stratum of the sun's atmosphere, which is still hotter than the photosphere, and on the outer boundary of this hot region there appears to be a shell of excessively faint cloud, part of which is to be seen in Mr. De la Rue's photographs of the eclipse of 1860. It probably extends the whole way round the sun; it is, therefore, very desirable that this faint shell, which seems to lie at a distance of eight or ten seconds of space from the edge of the sun's disc, should be observed."[1]

Mr. Stoney was led to the opinion that in the upper regions of the sun's sodium atmosphere, at which the reversal of the lines takes place, the temperature is lower than that of a Bunsen's burner. The winged appearance of some lines, especially those of hydrogen, led Mr. Stoney to associate that appearance with a great quantity of the substance to which the lines were due; the absence of wings, as in the case of sodium, indicating a small quantity. The absence of nitrogen and oxygen from the sun led to the conclusion that compound bodies exist in the sun. The reason they are not revealed in the spectrum, being that the masses of the molecules are too high to enable them to reach the cool parts of the sun's atmosphere.[2]

Absence of nitrogen and oxygen.

Here then ends the long parenthesis, which has extended over five Chapters, in which I have attempted to show how Spectrum Analysis helps us in the study of Solar Physics. So far we have confined ourselves to the method employed by Kirchhoff which deals with the average spectrum of the sun. The remaining part of this book will deal in the main with the results obtained up to the present time by the *new method*, one in which each minutest portion of the sun is examined separately. To this new method reference has already been made in Chap. viii.

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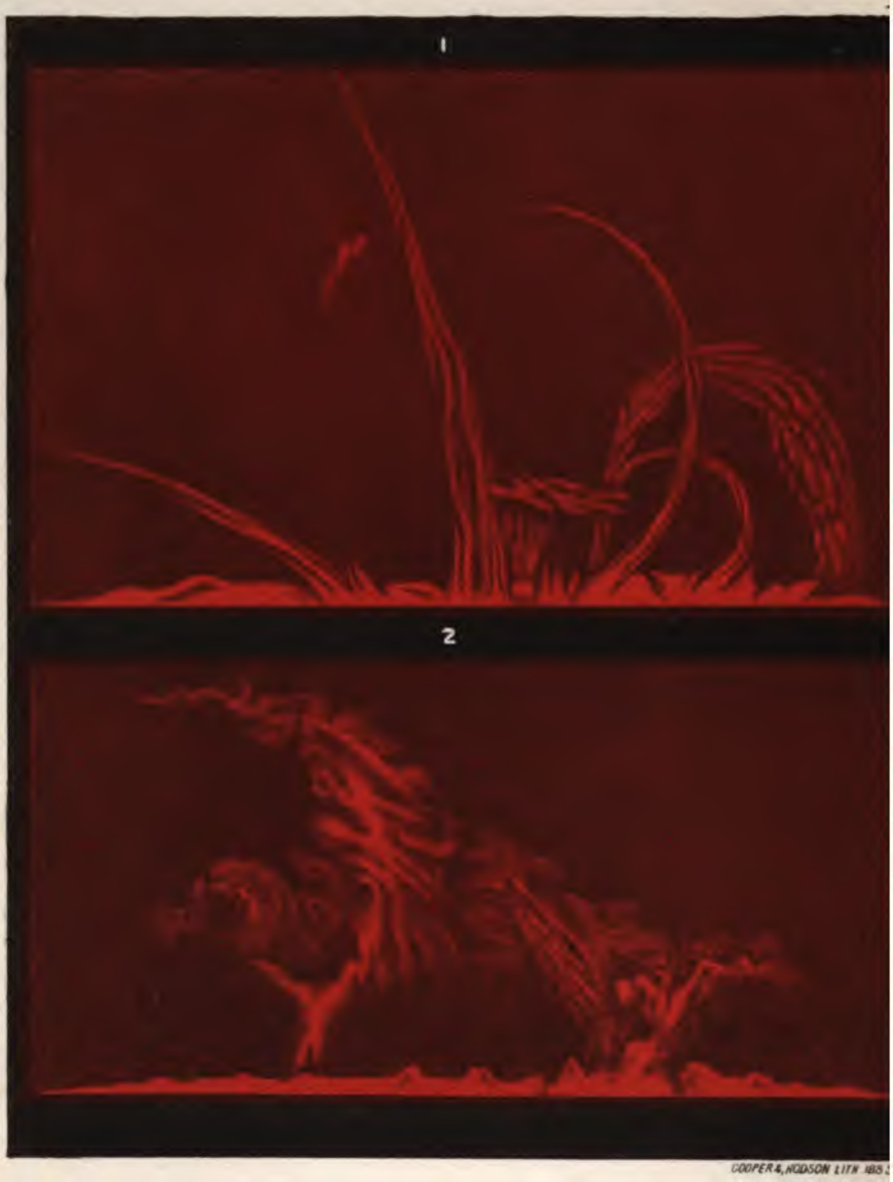
[1] *Monthly Notices*, vol. xxviii. p. 19. Nov. 1867.

[2] Proc. R.S. vol. xvii. p. 33.



COOPER & HODSON LITH 185

SOLAR PROMINENCES.

FROM THE DRAWINGS PUBLISHED BY THE DIRECTOR OF THE
HARVARD COLLEGE OBSERVATORY.

ENGLISH MILES

0 10 20 30 40 50 60 70 80 90 100,000

THE FIRST RESULTS OF THE NEW METHOD.[1]

IN the year 1865 two very important memoirs, dealing with all the telescopic and photographic observations accumulated up to that time on the subject of solar physics, were given to the world. One of them was privately printed in this country; the other appeared in the *Comptes Rendus* of the Paris Academy of Sciences.

Explanation of sun-spots.

I shall not detain you with a lengthened notice of these remarkable papers. I shall merely refer to the explanation given in both of them of the reason that a sun-spot appears dark—the very keystone of any hypothesis dealing with the physical constitution of the sun.

English science, represented by Messrs. De la Rue, Stewart, and Loewy, said that a spot is dark because the solar light is absorbed by a cool, non-luminous, absorbing atmosphere, pouring down there on to the visible surface of the sun—in other words, on to the photosphere.

French science, represented by M. Faye, said that a spot is dark because it is a hole in the photosphere, and the feebly luminous and therefore radiating interior gases of the sun are there alone visible.

Now most of you will see in a moment that here was a clear issue, which probably the spectroscope, and possibly nothing else, could solve; for the spectroscope is an instrument whose special *métier* it is to deal with radiation and absorption. It tells us that the light radiated from different bodies gives us spectra of different kinds, according to the nature of the radiating body,—continuous spectra without bright lines in the case of solids and liquids; and bright lines, with or without continuous spectra, in the case of gases and vapours. It tells us also that absorption dims the spectrum throughout its length when the absorption is *general*, and dims it here and there only when the absorption is *selective;* the well-known Fraunhofer lines being, as you will readily see, an instance of the latter kind. So that we have general and selective radiation, and general and selective absorption.

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[1] A lecture delivered at the Royal Institution of Great Britain, Friday evening, May 28th, 1869.

General and selective absorption.

Now then, with regard to the English theory, if there were more absorption in a spot than elsewhere, we might expect evidences of absorption; that is, the whole solar spectrum would be visible in the spectrum of a spot, but it would be dimmed, either throughout the length of the spectrum or in places only.

With regard to the French theory, having only radiating gaseous matter to deal with, we should, according to the then generally received idea, get bright lines only in the spot spectrum.

Here, then, was a tempting opportunity, and one which I considered myself free to use; for, although the spectroscope had then been employed—and you all know how nobly employed—for four years in culling secrets from stars and nebulæ, there was not, so far as I know, either published or unpublished observation on the sun, the nearest star to us. The field was therefore open for me, and I was not entering into another man's labour, when, on the 4th of March, 1866, I attached a small spectroscope to my telescope, in order to put the rival theories to a test, and thus bring another power to bear on a question which had remained a puzzle since it was first started by Galileo some two and a half centuries ago.

What I saw I will describe more fully by and by. It is sufficient here to mention that it was in favour of the English theory. There *was* abundant evidence of absorption in the spots, and there *was not* any indication of gaseous radiation.

Result.

Conception of new method.

Having then thus spectroscopically broken ground on the sun, a very natural inquiry was how next to employ this extension of a method of research, the discovery of which Newton had called, nearly 200 years before, "the oddest, if not the most considerable, detection which hath hitherto been made in the operations of nature."

There seemed one question which the spectroscope should now put to the sun above all others, and it was this:—

"Assuming this absorbing atmosphere to encircle the sun, in accordance with the general idea and Kirchhoff's hypothesis, what are those strange red flames seen apparently in it at total eclipses, jutting here and there from beyond the sun's hidden periphery, and here again hanging cloudlike?"

The tremendous atmosphere, which apparently the spectroscope had now proved to be a cool absorbing one, was supposed to be indicated during eclipses by a halo of light called the "corona," in which corona the red flames are visible. Now, as the red flames are always observed to give out more light than the corona, they were probably hotter than it; and reasoning thus on the matter with my friend Dr. Balfour Stewart one day, we came to the conclusion that they were most probably masses of glowing gas.

Now, this being so, the spectroscope *could* help us, and in this way.

Effect of prisms on light from solids and liquids,

The light from solid or liquid bodies, as you all I am sure know, is scattered broadcast, so to speak, by the prism into a long band of light, called a continuous spectrum, because from one end of it to the other the light is persistent.

and from gases.

The light from gaseous and vaporous bodies, on the contrary, is most brilliant in a few channels ; it is *husbanded*, and, instead of being scattered broadcast over a long band, is limited to a few lines in the band—in some cases to a very few lines.

Hence, if we have two bodies, one solid or liquid and the other gaseous or vaporous, which give out exactly equal amounts of light, then the bright lines of the latter will be brighter than those parts of the spectrum of the other to which they correspond in colour or refrangibility.

Separation of mixed spectra.

Again, if the gaseous or vaporous substance gives out but few lines, then, although the light which emanates from it may be much less brilliant than that radiated by a solid or liquid, the light may be so localized, and therefore intensified, in one case, and so spread out, and therefore diluted, in the other, that the bright lines from the feeble light source may in the spectroscope appear much brighter than the corresponding parts of the spectrum of the more lustrous solid body. Now here comes a very important point : supposing the continuous spectrum of a solid or liquid to be mixed with the discontinuous spectrum of a gas, we can, by increasing the number of prisms in a spectroscope, dilute the continuous spectrum of the solid or liquid body very much indeed, and the dispersion will not seemingly reduce the brilliancy of the lines given out by the gas ; as a consequence, the more dispersion we employ the brighter relatively will the lines of the gaseous spectrum appear.

Cause of invisibility of prominences.

The reason why we do not see the prominences every day in our telescopes is that they are put out by the tremendous brightness of our atmosphere near the sun, a brightness due to the fact that the particles in the atmosphere reflect to us the continuous solar spectrum. There is, as it were, a battle between the light proceeding from the prominences and the light reflected by the atmosphere, and, except in eclipses, the victory always remains with the atmosphere.

You will see, however, in a moment, after what I have said, that there was a possibility that if we could bring a spectroscope on the field we might turn the tide of battle altogether, assuming the prominences to be gaseous, as the reflected continuous spectrum might be dispersed almost into invisibility, the brilliancy of the prominence lines scarcely suffering any diminution by the process.

Attempt to see prominences in 1866 failed on account of insufficient dispersion.

The first attempt was made in 1866, a Herschel-Browning spectroscope being attached to my telescope, and the first and many succeeding attempts failed; there was not dispersion enough to dilute the spectrum of the regions near the sun sufficiently, and as a consequence the tell-tale lines still remained veiled and invisible. Nature's secrets were not to be wrested from her by a *coup de main*.

The year 1868 brought us to the now famous eclipse, to see which scientific men hastened from all civilized Europe to India. To this eclipse and its results I need only refer, as they have already been dwelt on at some length in this theatre; suffice it to say that in the eclipse the spectroscope did its duty, and that the gaseous nature of the prominences was put beyond all question.

Janssen's observations.

But there was a magnificent pendant to the eclipse, to which I must request your special attention. One of the observers, M. Janssen—a spectroscopist second to none—the representative, in that peaceful contest, of the Académie des Sciences and of the Bureau des Longitudes, was so struck with the brightness of the prominences rendered visible by the eclipse that, as the sun again lit up the scene, and the prominences disappeared, he exclaimed, "*Je reverrai ces lignes là!*" and, being prevented by clouds from putting his design into execution that same day, he rose next morning long before the sun, and as soon as our great luminary had risen from a bank of vapours, he succeeded in obtaining spectroscopic evidence of the protuberances he had seen surrounding the eclipsed sun the day before. During the eclipse M. Janssen had been uncertain even as to the number of lines he had observed, but he now by this new method at his leisure determined that the prominences

CHAP. XIV. were built up of hydrogen, this fact being indicated by the presence of two bright lines corresponding to the dark lines C and F in the ordinary solar spectrum.

Let me show you how this result was accomplished, by throwing an enlarged photograph of my telescope and

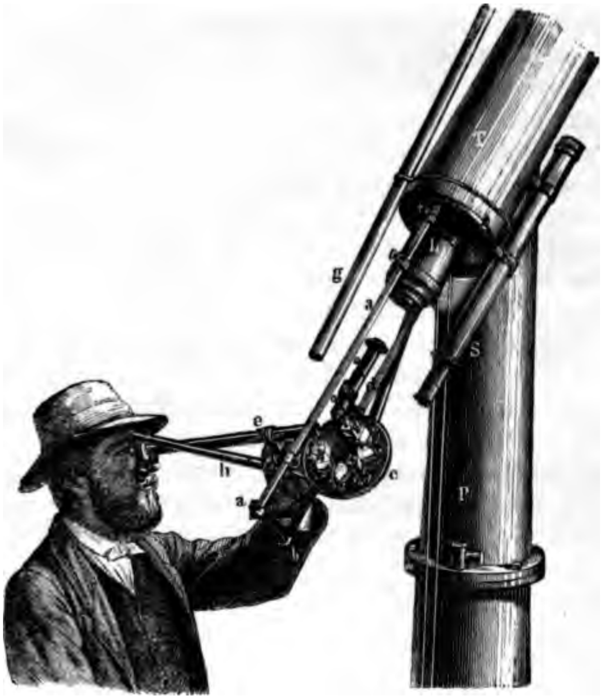

FIG. 84.—The spectroscope attached to the telescope for solar work. P, pillar of telescope; T, telescope; S, finder; L, eye-end of telescope: *a*, *a b*, supports of spectroscope; *d*, collimator; *c*, prism-plate; *e*, observing telescope; *h*, micrometer.

How observations were conducted. spectroscope on the screen. We have first the object-glass of the telescope, to collect the sun's rays and to form an image of the sun itself on a screen. In this screen is an excessively narrow slit, through which alone light can reach the spectroscope. This entering beam is grasped by another little object-glass and transformed into a cylinder[1] of light containing rays of all colours, which is now ready for its journey through the prisms. In its passage through them it is torn by each succeeding prism more out of its path, till at last, on emerging, it crosses the path it took on entering, and enters the little telescope you see, thoroughly dismembered but not disorganized.

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[1] Cylindrical, or nearly so, that is, in the case of each pencil.

Instead now of a cylinder of light containing rays of all colours, we have a cylinder of each ray which the little telescope compels to paint an image of the slit. Where rays are wanting, the image of the slit remains unpainted —we get a black line ; and when the telescope is directed to the sun, so that the narrow slit is entirely within the image of the sun, we get in the field of view of the little telescope a glorious coloured band with these dark lines crossing it.

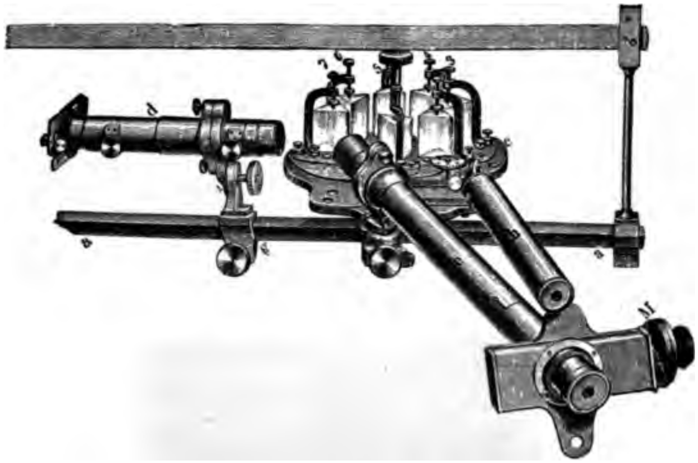

FIG. 85.—Enlarged view of spectroscope. *d*, collimator ; *e*, observing telescope ; *h* and M, two micrometers ; 1, 2, 3, 4, 5, 6, 7, prisms.

Place of slit on image.

Of course it is necessary for our purpose to allow only the *edge* of the sun to fall on the slit, leaving apparently a large portion of the latter unoccupied. What is seen, therefore,[1] is a very narrow band in the field of view of the telescope, and a large space nearly dark, as the dispersion of the instrument is so great that the atmospheric light is almost entirely got rid of, for a reason you are already acquainted with.

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[1] This refers to observations with a *radial* slit.

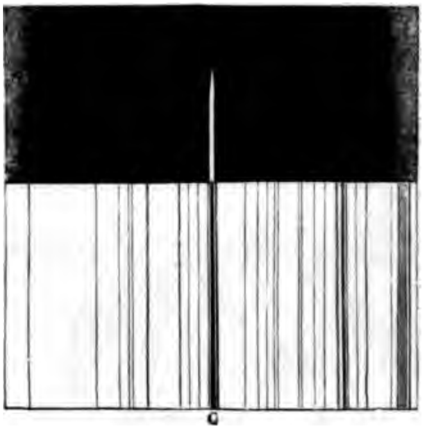

FIG 86.—Line C (red), with radial slit.

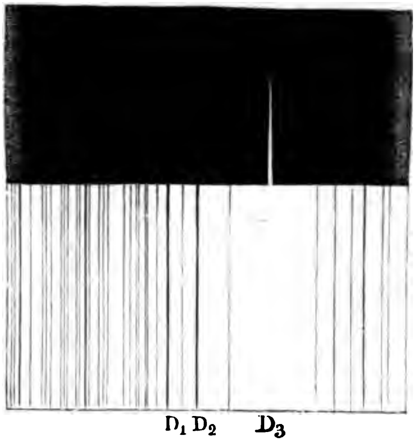

FIG. 87.—Line D3 (yellow), with radial slit.

Mr. Ladd will now show you on the screen what is seen when the slit reaches a prominence. First a line in the red, very obvious and brilliant, next a more delicate line in the yellow, then another in the green, and two others in the violet ; all these lines, with the exception of the yellow line, are in the positions occupied by known lines of hydrogen.

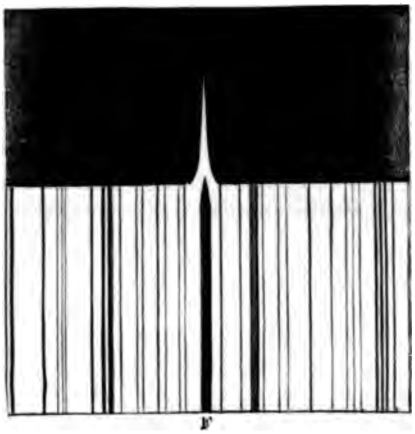

FIG. 88.—Line F (blue-green), with radial slit.

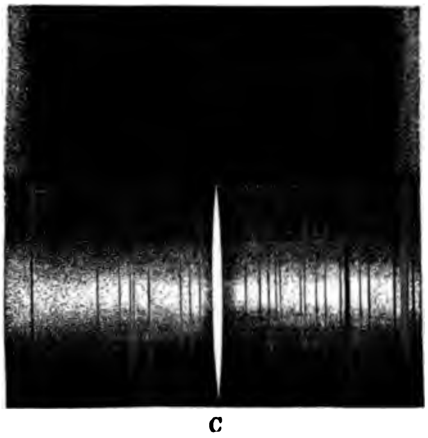

FIG. 89.—Line C, with tangential slit.

As the height of these bright lines must vary with the height of the prominences, and as the lines will only be visible where there is any hydrogen to depict them, it is obvious that the form of the prominences may be determined by confining the attention to one line, and slowly sweeping the slit over it.

First fruits.

The first fruits, then, of this new method of working with an uneclipsed sun was to tell us the actual composition of the prominences, and to enable us to determine their shapes and dimensions.

For the next steps you must permit me to refer more particularly to my own observations.

The Chromosphere.

When I was first able to obtain results in this country similar to those previously obtained by M. Janssen, though unknown to us, my instrument was incomplete; when other adjustments had been added by Mr. Browning, I found that at whatever part of the sun's edge I looked, I could not get rid of the newly-discovered lines. They were not so long as I had seen them previously, but there they were, not to be extinguished, showing that for some 5,000 miles in height all round the sun there was an envelope of which the prominences were but the higher waves. This envelope I named the "Chromosphere," as it is the region in which all the variously coloured effects are seen in total eclipses, and because I considered it of importance to distinguish between its discontinuous spectrum and the continuous one of the photosphere. And now another fact came out. The bright line F took the form of an arrow-head (see Fig. 88), the dark Fraunhofer line in the ordinary spectrum forming the shaft, the corresponding chromospheric line forming the head; it was broad close to the sun's edge, and tapered off to a fine point, an appearance not observed in the other lines.

Thickening of the F line.

Nature is always full of surprises, and here was a surprise and a magnificent help to further inquiry lurking in this line of hydrogen! MM. Plücker and Hittorf had already recorded that, under certain conditions, the green line of hydrogen widened out; and it at once struck me that the "arrow-head" was nothing but an indication of this widening out as the sun was approached.

I will now, then, for one moment leave the observatory work to say a word on some results recently obtained by Dr. Frankland and myself, in the researches on the radiation and absorption of hydrogen and other gases and vapours, upon which we have for some time been engaged.

First, as to hydrogen: what could laboratory work tell us about the chromosphere and the prominences?

It was obviously of primary importance—

1. To determine the cause to which the widening of the F line was due.

2. To study the hydrogen spectrum very carefully under varying conditions, with a view of detecting whether or not there existed a line in the orange.

F line is thickened by pressure.

We soon came to the conclusion that the principal, if not the only cause of the widening of the F line was *pressure*.

Having determined, then, that the phenomena presented by the F line were phenomena depending upon and indicating varying pressures, we were in a position to determine the atmospheric pressure operating in a prominence, in which the red and green lines are nearly of equal width, and in the chromosphere, through which the green line gradually expands as the sun is approached.

Higher prominences tenuous.

With regard to the higher prominences, we have obtained evidence that the gaseous medium of which they are composed exists in a condition of *excessive tenuity;* and that even at the lower surface of the chromosphere, that is, on the sun itself, in common parlance, the pressure is very far below the pressure of the earth's atmosphere.

Now I need hardly point out to you that the determination of the above-mentioned facts leads us necessarily

Bearing on Kirchhoff's theory.

to several important modifications of the received theory of the physical constitution of our central luminary—the theory which we owe to Kirchhoff, who based it upon his examination of the solar spectrum. According to his hypothesis, the photosphere itself is either solid or liquid, and it is surrounded by an extensive cool and non-luminous atmosphere composed of gases and the vapours of the substances incandescent in the photosphere.

An incandescent atmosphere surrounds the photosphere.

We find, however, instead of this compound cool and non-luminous atmosphere outside the photosphere, one which is in a state of incandescence, is therefore luminous, and which gives us merely, or at all events mainly, the spectrum of hydrogen; and the tenuity of this incandescent atmosphere is such that it is extremely improbable that any considerable atmosphere, such as the corona has been imagined to indicate, exists outside it.

Here already, then, we find the "cool absorbing atmosphere" of the theorists terribly reduced in height, and apparently much more simple in its composition than had been imagined by Kirchhoff and others. Dr. Frankland and myself have shown separately—

1. That a gaseous condition of the photosphere is quite consistent with its continuous spectrum, whether we regard the spectrum of the general surface or of spots. The possibility of this condition has also been suggested by Messrs. De la Rue, Stewart, and Loewy.

2. That a sun-spot is a region of greater absorption.

3. That when photospheric matter is injected into the chromosphere, we see bright lines.

4. That there are bright lines in the solar spectrum itself.

All these are facts which indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself *or extremely near to it*,[1] instead of in an extensive outer absorbing atmosphere. And this conclusion is strengthened by the consideration that otherwise the newly-discovered bright lines of hydrogen should themselves show traces of absorption on Kirchhoff's theory; but I shall show you presently that, so far from this being the case, they *appear bright actually in the very centre of the disc*, and, moreover, the vapours of sodium, iron, magnesium, and barium are often bright in the chromosphere, showing that they would always be bright there *if the vapours were always present*, as they should be on Kirchhoff's hypothesis; so that we may say that the photosphere *plus* the chromosphere is the real atmosphere of the sun, and that the sun itself is in such a state of fervid heat that the actual outer boundary of its atmosphere, *i.e.* the chromosphere, is in a state of incandescence.

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[1] I have italicised this passage in 1873, as some critics of my work have overlooked it.

With regard to the line in the orange I have nothing yet to tell. Dr. Frankland and myself are at the present moment working upon it.

I have next to take you a stage lower into the bowels,

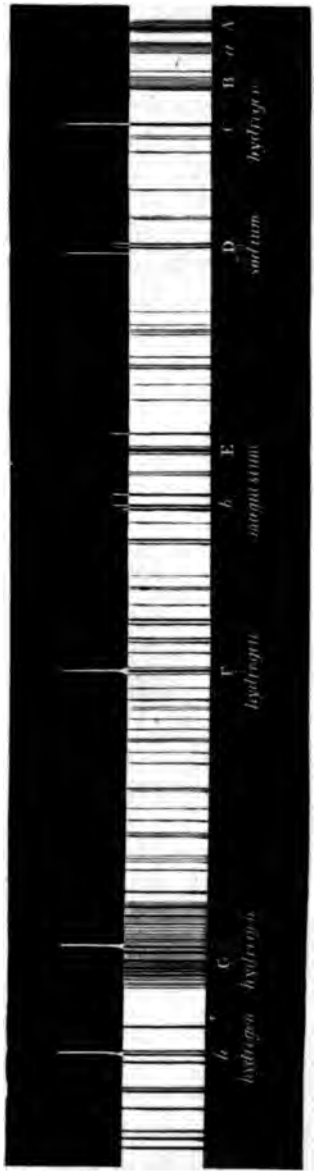

FIG. 90.—Spectrum of Chromosphere, showing the different lengths of the lines of hydrogen, sodium, and magnesium.

Convection currents in the atmosphere.

not of the earth, but of the sun. As a rule, the chromosphere rests conformably, as geologists would say, on the photosphere, but the atmosphere (as I have just defined it) is tremendously riddled by convection currents; and where these are most powerfully at work, the upper layers of the photosphere are injected into the chromosphere. Thus I have observed the lines due to the vapours of sodium, magnesium, barium, and iron in the spectrum of the chromosphere, appearing there as very short and very ***thin lines***, generally much thinner than the black lines due to their absorption in the solar spectrum.

Contortions of the hydrogen lines.

These injections are nearly always accompanied by the strangest contortions of the hydrogen lines, of which more presently. Sometimes during their occurrence the chromosphere seems full of lines, those due to the hydrogen towering above the rest.

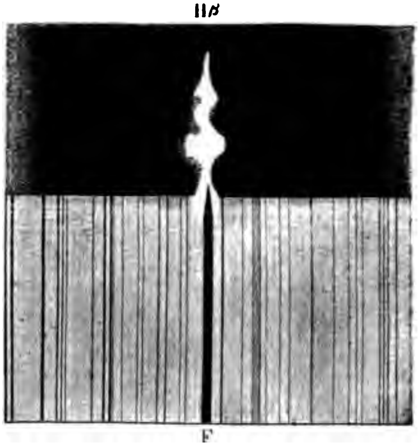

FIG. 91.—Contortions of F line in a prominence.

Attempt to see forms of prominences.

At the same time we have tremendous changes in the prominences themselves, which I have recently been able to see in all their beauty. I attempted to accomplish this in the first instance by means of an oscillating slit; but hearing that Mr. Huggins had succeeded in doing the same thing by means of absorptive media, using an open slit, it struck me at once that an open slit was quite sufficient, and this I find to be the case. By this method the smallest details of the prominences and of the chromosphere itself are rendered perfectly visible and easy of observation, and for the following reason. As you already know, the hydrogen Fraunhofer lines (like all the others) appear dark because the light which would otherwise paint an image of the slit in the place they occupy is absorbed; but when we have a prominence on the slit, there is light to paint the slit; and as in the case of any one of the hydrogen lines we are working with light of one refrangibility only, on which the prisms have no dispersive power, we may consider the prisms abolished. Further, as we have the prominence image coincident with the slit, we shall see it as we see the slit, and the wider we open the slit the more of the prominence shall we see. We may use either the red, or green, or blue light of hydrogen for the purpose of thus seeing the shape and details of the prominences; how far the slit may be opened depends upon the purity of the sky at the time.

Any hydrogen line may be used to see the prominences.

I have been perfectly enchanted with the sight which my spectroscope has thus revealed to me. The solar and atmospheric spectra being hidden, and the image of the wide slit and the part of the prominence under observation alone being visible,[1] the telescope or slit is moved slowly, and the strange shadow-forms flit past, and are seen as they are noticed in eclipses. Here one is reminded, by the fleecy, infinitely delicate cloud-films, of an English hedge-row with luxuriant elms; here of a densely intertwined tropical forest, the intimately interwoven branches threading in all directions, the prominences generally expanding as they mount upwards, and changing slowly, indeed almost imperceptibly.

It does not at all follow that the largest prominences are those in which the intensest action, or the most rapid change, is going on—the action as visible to us being generally confined to the regions just in, or above, the chromosphere; the changes arising from violent uprush or rapid dissipation—the uprush and dissipation representing the birth and death of a prominence. As a rule, the attachment to the chromosphere is narrow and is not often single; higher up, the stems, so to speak, intertwine, and the prominence expands and soars upward until it is lost in delicate filaments, which are carried away in floating masses.

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[1] This was accomplished by inserting a diaphragm with a small square aperture in the centre.

Forms of prominences.

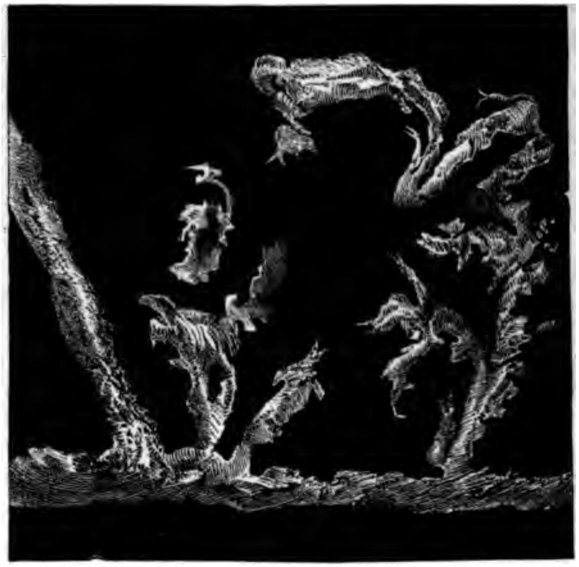

FIG. 92.—Prominence observed 14th March, 1869, 11h. 5m.

Since last October, up to the time of trying the method of using the open slit, I had obtained evidence of considerable changes in the prominences from day to day. With the open slit it is at once evident that changes on the small scale are continually going on; but it was only on the 14th of March that I observed any change at all comparable in magnitude and rapidity to those already recorded by M. Janssen.

About 9h. 45m. on that day, with the slit lying nearly along the sun's edge instead of across it as usual, I observed a fine dense prominence near the sun's equator, on the eastern limb, with signs of intense action going on. At 10h. 50m., when the action was slackening, I opened the slit and saw at once that the dense appearance had all disappeared, and cloud-like filaments had taken its place. The first sketch, now exhibited (Fig. 92), embracing an irregular prominence with a long perfectly straight one, was finished at 11h. 5m., the height of the prominence being 1′ 5″, or about 27,000 miles. I left the Observatory for a few minutes, and on returning, at 11h. 15m., I was astonished to find that the straight part of the prominence had entirely disappeared; not even the slightest rack appeared

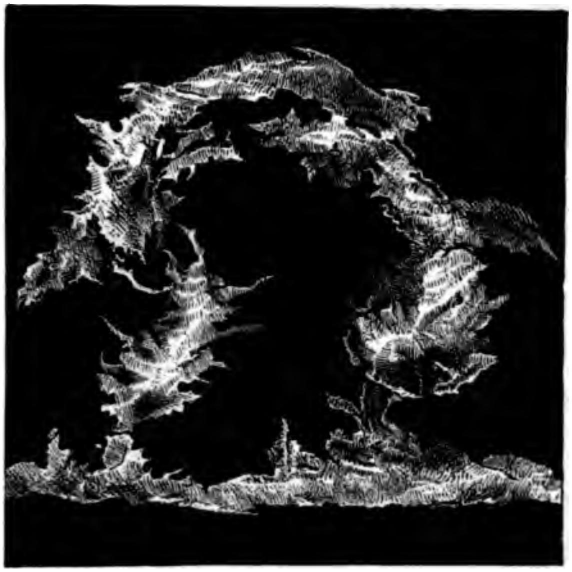

FIG. 93.—The same prominence, 11h. 15m.

Changes in prominences.

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in its place. Whether it was entirely dissipated, or whether parts of it had been wafted towards the other part, I do not know, although I think the latter explanation the more probable one, as the other part had increased.

So much, then, for the chromosphere and the prominences, which I think the recent work has shown to be the last layer of the true atmosphere of the sun. I shall now invite your attention to spots.

Thickening of lines in the spectrum of a spot.

Now, as a rule, precisely those lines which are injected into the photosphere by convection currents are most thickened in the spectrum of a spot, and the thickening increases with the depth of the spot, so that I no longer regard a spot simply as a cavity, but as a place in which principally the vapours of sodium, barium, iron, and magnesium occupy a lower level than they do ordinarily in the atmosphere.

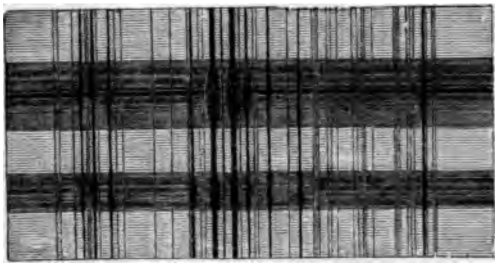

FIG. 94.—Spectrum of a sun-spot, showing the increased absorption of the sodium vapour gradual in the case of a spot with shelving, and sudden in the case of a spot with steep, sides.

I have told you before, that when these lines are observed in the chromosphere, they are usually thinner than their Fraunhofer lines.

Spot-spectrum.

I will now show a photograph of a spot-spectrum on the screen. You will see a black band running across the ordinary spectrum ; that black band indicates the general absorption which takes place in a sun-spot. Now mark the behaviour of the Fraunhofer lines ; see how they widen as they cross the spot, putting on a sudden blackness and width in the case of a spot with steep sides, expanding gradually in a shelving one. The behaviour of these lines is due to selective absorption.

We have, then, the following facts: mark them well:—

1. The lines of sodium, magnesium, and barium, when observed in the chromosphere, are among those which are thinner than their usual Fraunhofer lines.

2. The lines of sodium, magnesium, and barium, when observed in a spot, are among those which are thicker than their usual Fraunhofer lines.

They show, I think, that a spot is the seat of a downrush or downsinking.

Downrush seen telescopically in 1865.

Messrs. De la Rue, Stewart, and Loewy, who brought forward the theory of a downrush before my observations of an actual downrush were made in 1865, at once suggested as one advantage of this explanation, that all the gradations of darkness, from the faculæ to the central umbra, may be supposed to be due to the same cause; namely, the presence to a greater or less extent of a relatively cooler absorbing atmosphere; thus suggesting as one cause of the darkening of a spot—

1. The general absorption of the atmosphere, thicker here than elsewhere, as the spot is a cavity.

To which the spectroscope added in 1866, as you know—

2. Greater selective absorption.

Absorption by sodium vapour.

I have Dr. Frankland's permission to exhibit an experiment connected with our researches on absorption which will show you that this increased selective absorption can be fairly grappled with in our laboratories. I will show you on the screen the absorption line due to sodium vapour, in one part as thin as it is in the ordinary solar spectrum; in another, almost if not quite as thick as it appears in a spot; and I accomplish this result in the following way:— Here I have an electric lamp, and by means of this slit I only permit a fine line of light to emerge from it; here the beam passes through a bisulphide of carbon prism, and there you see on the screen the glorious spectrum, due to the dismemberment of the fine line of polychromatic light. Mr. Pedler will now place a glass tube containing metallic sodium, sealed up with hydrogen, in front of the slit, and will heat it with a spirit lamp.

As the sodium vapour rises you see the dark line of absorption make its appearance as an extremely fine line, and finally you see that the light which traverses the upper layer of the sodium vapour scarcely suffers any absorption—the line is thin ; while, on the contrary, the light which has traversed the lower, denser layers has suffered tremendous absorption : the line is inordinately thick, such as we see it in the spectrum of a spot.

So much, then, for the selective absorption. My recent observations, to which I will shortly draw attention, show, I think, that it is of great importance, especially in connection with the fact that the passage from the penumbra to the umbra is generally less gradual than that from the photosphere to the penumbra. You see now how much is included in the assertion that the photosphere is gaseous.

You are all, I know, familiar with that grand generalization of Kirchhoff's, by which he accounted for the Fraunhofer lines.

If we have a gas or a vapour less luminous than another light-source, and view that light-source through the gas or vapour, then we shall observe absorption of those particular rays which the gaseous vapour would emit if incandescent.

Let us confine our attention to the hydrogen Fraunhofer lines.

When I observe the chromosphere on the sun's limb, with no brighter light-source behind it, I observe its characteristic lines *bright*. But when I observe them on the sun itself—that is, when the brighter sun is on the other side of the hydrogen envelope, then, as a rule, its function is reduced—is toned down—the envelope acts as an absorber—the lines are observed black.

Hydrogen lines seen bright upon the sun itself.

Now what must we conclude when I tell you that, at the present time, it is almost impossible to observe the sun for an hour without observing the hydrogen lines, every now and then, *bright upon the sun itself!*

Not only are the lines observed bright, but it would appear that the strongly luminous hydrogen is carried up by the tremendous convection currents at different pressures; and under these circumstances the bright line is seen to be expanded on both sides of its normal position. Moreover, at times there is a dim light on both sides the black line, and the line itself is thinned out, showing that, although there is an uprush of strongly luminous material, the column is still surmounted by some less luminous hydrogen, possibly separated from the other portion, which still performs the functions of an absorber. This seems established by another fact, namely that at times the lines, still black, expand on both sides, as if, in fact, in these regions there were a depression in the chromosphere; you already know that the pressure is greater at the base of the chromosphere than at the summit.

Line F most sensitive to pressure effects.

For this reason it is best to observe these phenomena by means of the green line, which expands in a more decided manner by pressure than does the red.

Movements of the chromosphere.

I now come to a new field of discovery opened out by these investigations, a branch of the inquiry which I fear you will consider more startling than all the rest—a branch, however, which I have had many opportunities of studying, and which has required me to move with the utmost caution. I allude to the movements of the hydrogen envelope and prominences at which I have before hinted.

Anyone who has observed the sun with a powerful telescope, especially in a London fog—all too great a rarity unfortunately for such work—will have been struck with the tremendous changes observed in spots. Now, change means movement; and as spot phenomena occur immediately below the level of the chromosphere, we may easily imagine that the chromosphere and its higher waves, the prominences, will also partake of the movements, be they up- or down-rushes, cyclones, or merely lateral motions. I have thrown on the screen a photograph of a drawing of a sun-spot observed under the clear sky of Rome by Father Secchi—a drawing I regard as a most faithful counterpart of nature.

You see how the photosphere is being driven about and contorted; how here it seems to be torn to ribbons by the action of some tremendous force, how here it is dragged down and shivered to atoms.

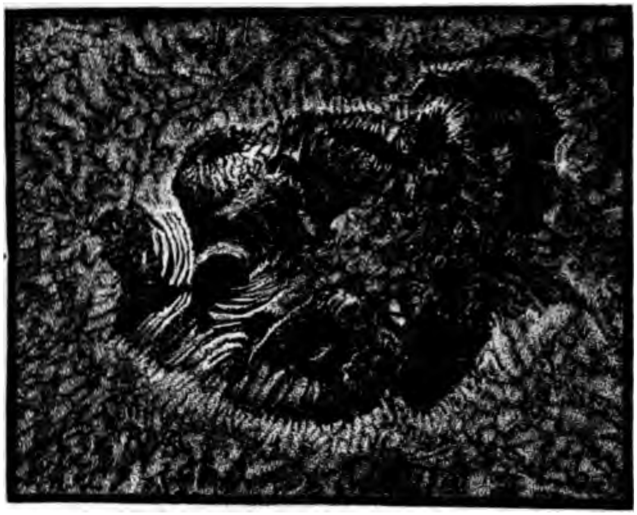

FIG. 95.—Sun-spot. (Secchi.)

Spectroscope determines rate of movement.

The spectroscope enables us to determine the velocities of these movements with a considerable approach to accuracy; and at times they are so great that I am almost afraid to mention them to you.

Let me first endeavour to give you an idea how this result is arrived at, and I must here beg your indulgence for a gross illustration of one of the most supremely delicate of Nature's operations.

Imagine a barrack out of which is constantly issuing with measured tread and military precision an infinite number of soldiers in single or Indian file; and suppose yourself in a street seeing these soldiers pass. You stand still, and take out your watch, and find that so many pass you in a second or minute, and that the number of soldiers, as well as the interval between them, is always the same.

Change of refrangibility by a motion of advance or retreat explained.

You now move slowly towards the barrack, still noting what happens. You find that more soldiers pass you than before in the same time, and, reckoned in time, the interval between each soldier is less.

You now move still slowly from the barrack, *i.e.* with the soldiers. You find that fewer soldiers now pass you, and that the interval between each is longer.

Now suppose yourself at rest, and suppose the barrack to have a motion now towards you, now from you.

In the first case the men will be payed out, so to speak, more rapidly. The motion of the barrack-gate towards you will plant each soldier nearer the preceding one than he would have been if the barrack had remained at rest. The soldiers will really be nearer together.

In the second case it is obvious that the interval will be greater, and the soldiers will really be further apart.

So that, generally, representing the interval between each soldier by an elastic cord, if the barrack and the eye approach each other by the motion of either, the cord will contract; in the case of recession, the cord will stretch.

Now let the barrack represent the hydrogen on the sun, perpetually paying out waves of light, and let the elastic cord represent one of these waves; its length will be changed if the hydrogen and the eye approach each other by the motion of either.

Particular wave-lengths with the normal velocity of light are represented to us by different colours.

The long waves are red.

The short waves are violet.

Now let us fix our attention on the green wave, the refrangibility of which is indicated by the F line of hydrogen. If any change of wave-length is observed in this line, *and not in the adjacent ones*, it is clear that it is not to the motion of the earth or sun, but to that of the hydrogen itself and alone that the change must be ascribed.

If the hydrogen on the sun is approaching us, *the waves will be crushed together;* they will therefore be shortened, and the light will incline towards the violet, that is, towards the light with the shortest waves; and if the waves are shortened only by the $\frac{1}{10,000,000}$th of a millimetre, we can detect the motion.

If the hydrogen on the sun is receding from us, the waves will be drawn out; they will therefore be longer, and the green ray will incline towards the red.

Conditions of approach or recess.

I must next point out, that there are two different circumstances under which the hydrogen may approach or recede from the eye.

I have here a globe, which we will take as representing the sun. Fix your attention on the centre of this globe;[1] it is evident that an uprush or a downrush is necessary to cause any alteration of wave-length. A cyclone or lateral movement of any kind is powerless; there will be no motion to or from the eye, but only at right angles to the line of sight.

Next fix your attention on the edge of the globe—the limb, in astronomical language; here it is evident that an upward or downward movement is as powerless to alter the wave-length as a lateral movement was in the other case, but that, should any lateral or cyclonic movement occur here of sufficient velocity, it might be detected.

So that we have the centre of the disc for studying upward and downward movements, and the limb for studying lateral or cyclonic movements, if they exist.

If the hydrogen-lines were invariably observed to broaden out on both sides, the idea of movement would

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[1] *I.e.* the middle point of the hemisphere turned to each observer.

Change not due to pressure, as it is not the same on both sides of the line.

require to be received with great caution: we might be in presence of phenomena due to greater pressure, both when the lines observed are bright or black upon the sun; but when they widen out, sometimes on one side, sometimes on the other, and sometimes on both, this explanation appears to be untenable, as Dr. Frankland and myself in our researches at the College of Chemistry have never failed to observe a widening out, equally or nearly so, on both sides the F line when the pressure of the gas has been increased.

You see now on the screen (Fig. 96) a diagram showing the strange contortions which the F hydrogen line undergoes at the centre of the sun's disc. Not only have we the line bright, as I have before told you, but the dark one is twisted in places, generally inclining towards the red; and often when this happens we have a bright line on the violet side. You see it (Fig. 97), sometimes, stopping short of one of the small sun-spots; swelling out prior to disappearance; invisible in a facula between two small spots; changed into a bright line, and widened out on both sides two or three times in the very small spots; becoming bright near a spot, and expanding over it on both sides; very many times widened out near a spot, sometimes considerably, on the less refrangible side; and, finally, extended as a bright line without any thickening over a small spot.

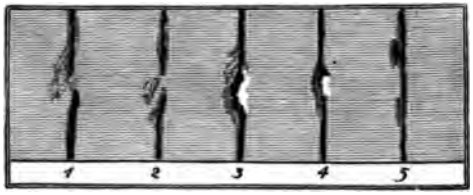

FIG. 96.—Contortions of F line on disc. 1 and 2, rapid downrush and increasing temperature; 3 and 4, uprush of bright hydrogen and downrush of cool hydrogen; 5, local downrushes associated with hydrogen at rest.

Measurement of velocities of movement.

Now the other Fraunhofer lines on the diagram (Fig. 98) may be looked upon as so many milestones telling us with what rapidity the uprush and downrush take place; for these twistings are nothing more or less than alterations of wave-length, and thanks to Ångström's map we can map out distances along the spectrum from F in $\frac{1}{10,000,000}$ths of a millimetre from the centre of that line; and we know that an alteration of that line $\frac{1}{10,000,000}$th mm. towards the violet means a velocity of 38 miles a second towards the eye, *i.e.* an uprush; and that a similar alteration towards the red means a similar velocity from the eye, *i.e.* a downrush. The fact that the black line inclines to the red shows that the less bright hydrogen descends; the fact that the bright line—where both are visible side by side—inclines to the violet, shows that the more vivid hydrogen ascends; and the alteration of wave-length is such that 20 miles a second is very common.

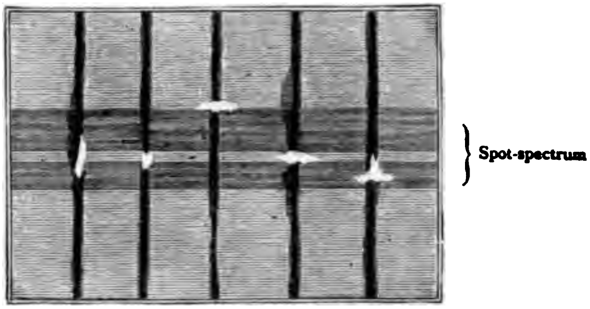

FIG. 97.—Contortions of F line on disc, in connection with spots and uprushes of bright hydrogen.

Velocities at limb much greater than on disc.

Now, observations of the lateral motions at the limb are of course made by the chromospheric bright lines seen beyond the limb. Here the velocities are very much more startling; not velocities of uprush and downrush, as you now know, but swinging and cyclonic motions of the hydrogen.

I will first show you a cyclone observed on the 14th of March, but before I do so let me make one remark. Although the slit used is as narrow as I can make it, let us say $\frac{1}{500}$th—I have not measured it—of an inch, a strip of this breadth, of the sun's image, is something considerable, as the glorious sun himself is painted by my object-glass only about ·94 inch in diameter, so that after all the slit lets in to be analysed a strip some 1,800 miles wide.

Size of the portion of the sun included in the slit.

Now, suppose we have a cyclone of incandescent hydrogen some 1,500 miles wide tearing along with a very rapid rotatory motion, it is clear that all this cyclone could fall within the slit; and that if the rotatory motion were sufficiently rapid, the spectroscope should separate the waves which are carried towards us from those which are receding.

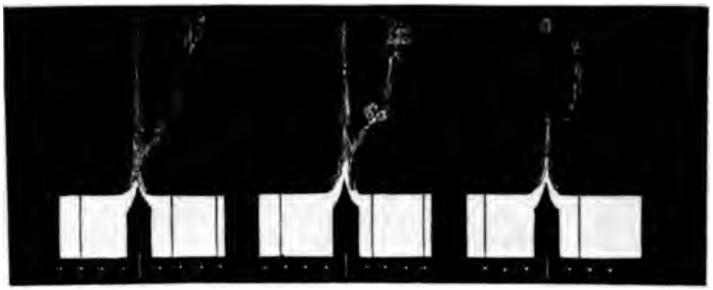

FIG. 98.—Alterations of wave-length in prominences. The dots show $\frac{1}{10000000}$mm.

It does this: as you see, we have an alteration of wave-length both towards the red and violet, amounting to something like 40 miles a second. Now it should be clear to you that, by moving the slit first one way and then the other, we may be able to bring it in turn to such positions that only the light proceeding from either side of the cyclone can enter it. Then we shall have changes of wave-length in one direction only, in each case precisely as you see was observed.

Now, let us suppose that, instead of a cyclone, we have a motion of some portions of the prominence towards the eye; and that, moreover, the rate of motion varies exces-

Simultaneous movements.

sively in some portions. What we shall see will be this. The portion of the prominence at rest will give us no alteration of wave-length; its bright line will be in a line with the corresponding black one in the spectrum. The portion moving towards the eye, however, will give us an alteration of wave-length towards the violet. You are now in a position to grasp the phenomena revealed to me by my spectroscope on the 12th instant, when at times the F line was triple! the extreme alteration of wave-length being such that the motion of that part of the prominence giving the most extreme alteration of wave-length must have exceeded 120 miles per second, if we are to explain these phenomena by the only known possible cause which is open to us.

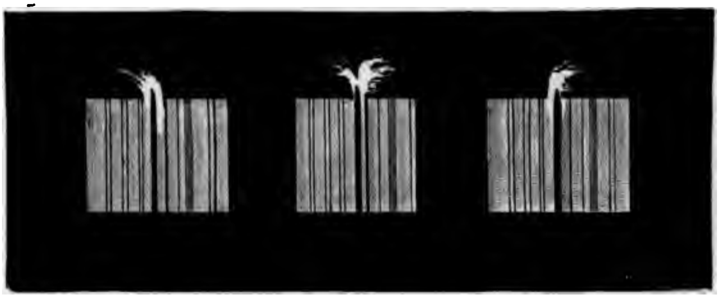

FIG. 99.—Solar cyclone. Left-hand diagram, retreating side of cyclone on slit; centre diagram, both sides on slit; right-hand diagram, advancing side on slit.

By moving the slit it was possible to see in which part of the prominence these great motions arose, and to follow the change of wave-length to its extremest limit.

Connection of spectroscopic and telescopic changes.

By the kindness of Dr. Balfour Stewart, I am able to exhibit to you some of the Kew sun-pictures which show you how these spectroscopic changes are sometimes connected with telescopic ones.

On the 21st April there was a spot very near the limb which I was enabled to observe continuously for some time. At 7.30 A.M. there was a prominence visible in the field of view, in which tremendous action was evidently going on, for the C, D, and F lines were magnificently bright in the ordinary spectrum itself, and as the spot-spectrum was also visible, it was seen that the prominence was in advance of the spot. The injection into the chromosphere surpassed anything I had seen before, for there was a magnesium cloud quite separated from the limb, and high up in the prominence itself.

A magnesium cloud observed above the chromosphere.

By 8.30 the action had quieted down, but at 9.30 another throb was observed, and the new prominence was moving away with tremendous velocity. While this was going on, the hydrogen lines suddenly became bright on the other side (the earth's side) of the spot, and widened out considerably—indeed to such an extent that I attributed their action to a cyclone, although, as you know, this was a doubtful case.

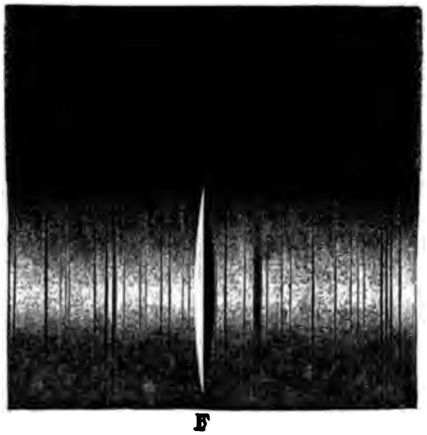

FIG. 100.—Non-coincidence of bright and dark F line when tangential slit is used, and when both photospheric and chromospheric light is admitted.

Now, what said the photographic record? The sun was photographed at 10h. 55m. A.M., and I hope you will be able to see on the screen how the sun's surface was disturbed near the spot. A subsequent photograph at 4h. 1m. P.M. on the same day shows the limb to be actually broken in that particular place: the photosphere seems to have

The limb of the sun broken in the photograph at the place where the cyclone was observed.

been absolutely torn away behind the spot, exactly where the spectroscope had afforded me possible evidence of a cyclone!

In connection with the last branches of the research I have brought to your notice, I may remark that we have two very carefully prepared recent maps of the solar spectrum, one by Kirchhoff, the other by Ångström, made a few years apart and at different epochs with regard to the sun-spot period. If you look at these maps, you will see a vast difference in the relative thicknesses of the C and F lines, and great differences in the relative darkness and position of the lines; and if I had time, I could show you that we now may be supplied with a barometer, so to speak, to measure the varying pressures in the solar and stellar chromospheres; for, depend upon it, every star has, has had, or will have, a chromosphere, and there are no such things as "worlds without hydrogen," any more than there are stars without photospheres. I suggested in 1866 that possibly a spectroscopic examination of the sun's limb might teach us somewhat of the outburst of the star in Corona, and already we see that all that is necessary to get just such an outburst in our own sun is to increase

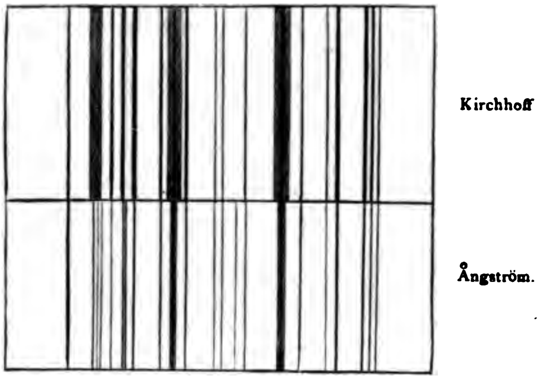

FIG. 101.—Comparison of *b* and adjacent lines, as mapped by Kirchhoff and Ångström.

Application of the results of solar study to the fixed stars.

the power of his convection currents, which we know to be ever at work. Here, then, is one cataclysm the less in astronomy—one less "World on Fire," and possibly also a bright light thrown on the past history of our own planet.

Application to variable stars.

I might show you further that we now are beginning to have a better hold on the strange phenomena presented by variable stars, and that an application of the facts I have brought to your notice this evening, taken in connection with the various types of stars[1] which have been indicated by Father Secchi with admirable philosophy, opens out generalizations of the highest interest and importance; and that, having at length fairly grappled with some of the phenomena of the nearest star, we may soon hope for more certain knowledge of the distant ones.

At present, however, we may well leave speculation for those who prefer it to acquiring facts; let us rather, emboldened by the work which this new method of research has enabled us to accomplish in this country, under the worst atmospheric conditions, in seven short months, go on quietly deciphering one by one the letters of this strange hieroglyphic language which the spectroscope has revealed to us—a language written in fire on that grand orb which to us earth-dwellers is the fountain of light and heat, and even of life itself.

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[1] When this was written I did not know that Mr. Rutherford was the first to suggest this field of research.

THE AMERICAN ECLIPSE, 1869.

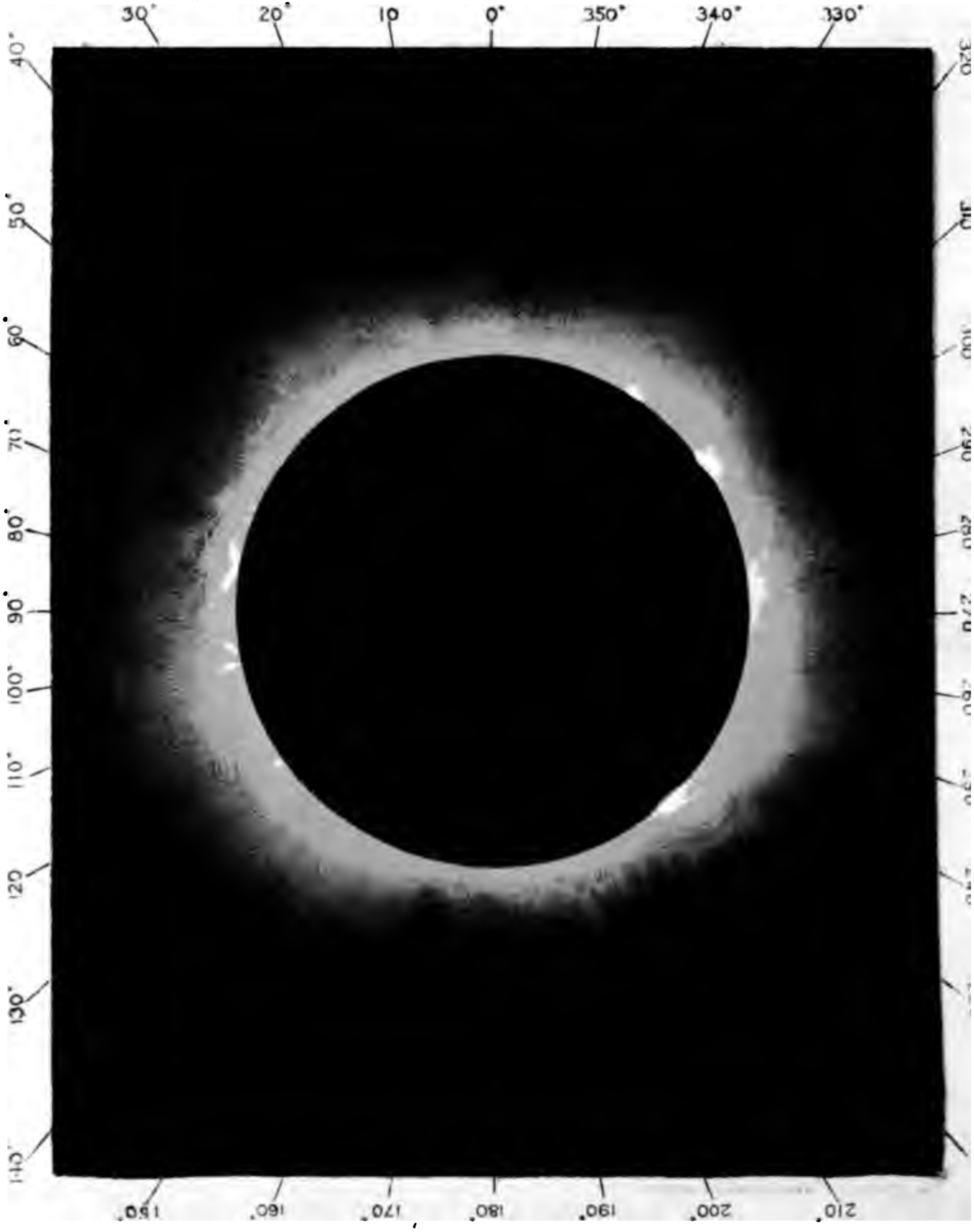
I.—FIRST IMPRESSIONS.

CHAP. XV. IF our American cousins in general hesitate to visit our little island, lest, as some of them have put it, they should fall over the edge; those more astronomically inclined may very fairly decline, on the ground that it is a spot where the sun steadily refuses to be eclipsed. This is the more tantalizing, because the Americans have just observed their third eclipse this century, and already I have been invited to another, which will be visible in Colorado, four days' journey from Boston (I suppose I am right in reckoning from Boston?) on July 29, 1878.

Thanks to the accounts in *Silliman's Journal* and the *Philosophical Magazine*, and to the kindness of Professors Winlock and Morton, who have sent me some exquisite photographs, I have a sufficient idea of the observations of this third eclipse, which happened on the 7th of August last, to make me anxious to know very much more about them—an idea sufficient also, I think, to justify some remarks on what we already know.

A few words are necessary to show the work that had to be done.

An eclipse of the sun, so beautiful and yet so terrible to the mass of mankind, is of especial value to the astronomer, Pl III to face page 240



THE PHOTOGRAPHIC CORONA 1869
TAKEN AT SHELBYVILLE KENTUCKY.

because at such times the dark body of the moon, far outside our atmosphere, cuts off the sun's light from it, and round the place occupied by the moon and moon-eclipsed sun there is therefore none of the glare which we usually see—a glare caused by the reflection of the sun's light by our atmosphere. If, then, there were anything surrounding the sun ordinarily hidden from us by this glare, we ought to see it during eclipses.

In point of fact, strange things are seen. There is a strange halo of pearly light visible, called the corona, and there are strange red things, which have been called red flames or red prominences, visible nearer the edge of the moon—or of the sun which lies behind it.

Halley supposed that the red flames belonged to the moon.

Now, although we might, as I have pointed out, have these things revealed to us during eclipses if they belonged to the sun, it does not follow that they belong to the sun because we see them. Halley, a century and a half ago, was, I believe, the first person to insist that they were appearances due to the moon's atmosphere, and it is only within the last decade that modern science has shown to everybody's satisfaction—by photographing them, and showing that they were eclipsed as the sun was eclipsed, and did not travel with the moon—that the red prominences really do belong to the sun.

The evidence with regard to the *corona* was not quite so clear, but I do not think I shall be contradicted when I say, that prior to the Indian eclipse last year the general notion was that the corona[1] was nothing more nor less than the atmosphere of the sun, and that the prominences were things floating in that atmosphere.

While astronomers had thus been slowly feeling their way, the labours of Wollaston, Fraunhofer, Herschel, Fox Talbot, Wheatstone, Kirchhoff, and Bunsen were providing them with an instrument of tremendous power, which was to expand their knowledge with a suddenness almost startling, and give them previously undreamt-of powers

[1] See note on page 243.

of research. I allude to the spectroscope, which was first successfully used to examine the red flames during the eclipse of last year. That the red flames were composed of hydrogen, and that the spectroscope enabled us to study them day by day, were facts acquired to science independently by two observers many thousand miles apart.

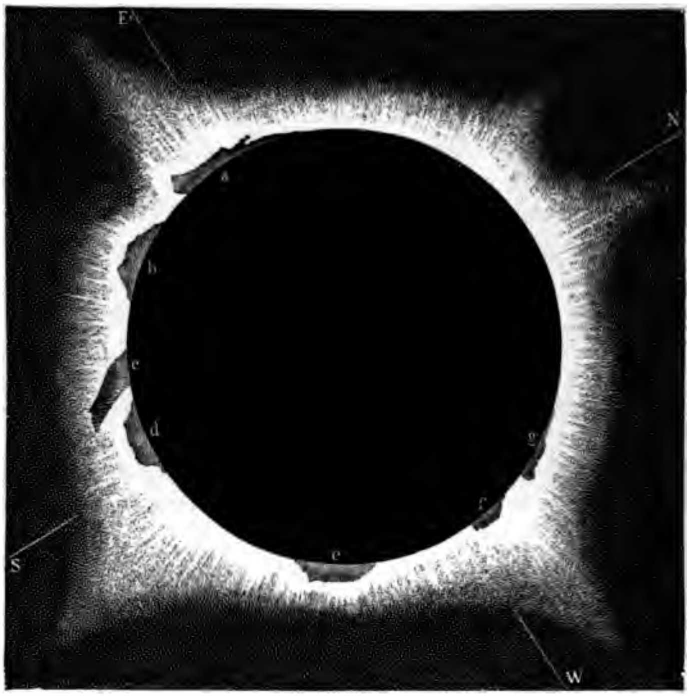


FIG. 102.—The Eclipsed Sun, August 1869, showing the corona and prominences at *a*, *b*, *c*, *d*, *e*, *f*, *g*, as observed at Des Moines.

The red flames were "settled," then, to a certain extent; but what about the corona?

After I had been at work for some time on the new method of observing the red flames, and after Dr. Frankland and myself had very carefully studied the hydrogen spectrum under previously untried conditions, we came to

Frankland and Lockyer's conclusions and reservation.

the conclusion that the spectroscopic evidence brought forward, both in the observatory and in the laboratory, was against any such *extensive* atmosphere as the corona had been imagined to indicate; and we communicated our conclusions to the Royal Society. Since that time, I confess, the conviction that the corona[1] is nothing else than an effect due to the passage of sunlight through our own atmosphere near the moon's place has been growing stronger and stronger; *but there was always this consideration to be borne in mind, namely, that as the spectroscopic evidence depends mainly upon the brilliancy of the lines, that evidence was in a certain sense negative only, as the glare might defeat the spectroscope with an uneclipsed sun in the coronal regions, where the temperature and pressure are lower than in the red-flame region.*

Point to be settled.

The great point to be settled then, in America, was, What is the corona? and there were many less ones. For instance, by sweeping round the sun with the spectroscope, both before and after the eclipse, and observing the prominences with the telescope merely during the eclipse, we should get a sort of key to the strange cypher-band called the spectrum, which might prove of inestimable value, not only in the future, but in a proper understanding of all the telescopic observations of the past. We should, in fact, be thus able to translate the language of the spectroscope. Again, by observing the spectrum of the same prominence both before and during, or during and after the eclipse, the effect of the glare on the visibility of the lines could be determined—but I confess I should not like to be the observer charged with such a task.

The corona a permanent solar aurora.

What, then, is the evidence furnished by the American observers on the nature of the corona? It is *bizarre* and puzzling to the last degree! The most definite statement on the subject is, that it is nothing more nor less than a

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[1] This word was applied when it was written to the coronas one or two solar diameters high, with rays some degrees long; and not, as some have asserted, to the coronas which are seen before and after totality.

CHAP. XV.

permanent solar aurora! the announcement being founded on the fact, that three bright lines remained visible after the image of a prominence had been moved away from the slit, and that one (if not all) of these lines is coincident with a line (or lines) noticed in the spectrum of the *aurora borealis* by Professor Winlock.

Now it so happens that among the lines which I have observed up to the present time—some forty in number—this line is among those which I have most frequently

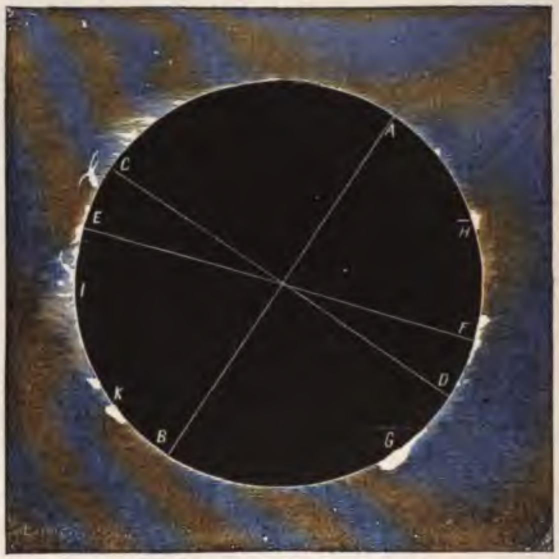

FIG. 103.—Copy of a photograph of the American Eclipse of August 7, 1869, obtained by Professor Morton's party. E, F, Moon's path.

The green coronal line.

recorded; it is, in fact, the first iron line which makes its appearance in the part of the spectrum I generally study when the iron vapour is thrown into the chromosphere. Hence I think that I should always see it if the corona were a permanent solar aurora, and gave out this as its brightest line; and on this ground alone I should hesitate to regard the question as settled, were the new hypothesis less startling than it is. The position of the line is approximately shown in the woodcut (Fig. 90) near E, together with the other lines more frequently seen.

A continuous spectrum given by the corona.

It is only fair, however, to Professor Young, to whom is due this important observation, to add that Professor Harkness also declares for one bright line in the spectrum of the corona, but at the same time he, Professor Pickering, and indeed others, state its spectrum to be also continuous, a remark hard to understand unless we suppose the slit to have been wide, and the light faint; in either of which cases final conclusions can hardly be drawn either way.

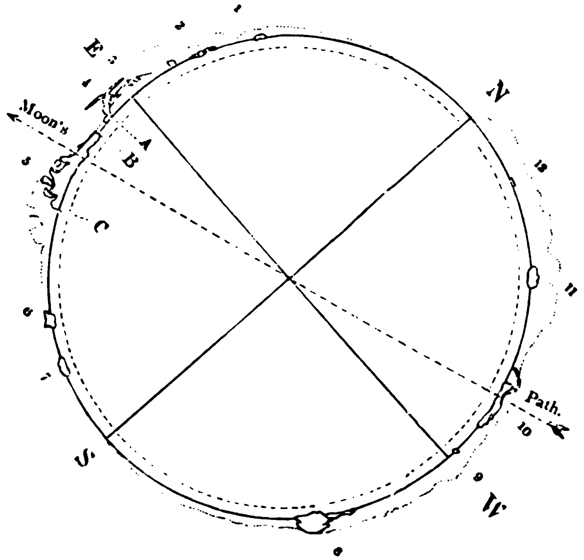

FIG. 104.—General view of the protuberances, American Eclipse, August 7th, 1869.

Pickering's polarization observations.

So much, then, for the spectroscopic evidence with which we are at present acquainted on the most important point. The results of the other attacks on the same point are equally curious and perplexing. Formerly, a favourite argument has been that because the light of the corona is polarized, therefore it is solar. The American observers state that the light is *not* polarized[1]—a conclusion, as M. Faye has well put it, "very embarrassing for Science." Further,—stranger still if possible,—it is stated that another line of inquiry goes to show that, after all, Halley may be right, and that the corona may really be due to a lunar atmosphere!

Question of corona not settled.

I think I have said enough to show that the question of the corona is by no means settled, and that the new method has by no means superseded the necessity of carefully studying eclipses; in fact, their observation has become of much greater importance than before; and I earnestly hope that all future eclipses in the civilized area in the old world will be observed with as great earnestness as the last one was in the new. Certainly, never before was an eclipsed sun so thoroughly tortured with all the instruments of Science. Several hundred photographs were taken, with a perfection of finish which may be gathered from the accompanying reproduction of one of them in Fig. 103.

The Government, the railway and other companies, and private persons threw themselves into the work with marvellous earnestness and skill; and the result was that the line of totality was almost one continuous observatory, from the Pacific to the Atlantic. We read in ***Silliman's Journal***, "There seems to have been scarcely a town of any considerable magnitude along the entire line, which was not garrisoned by observers, having some special astronomical problem in view." This was as it should have been, and the American Government and men of science must be congratulated on the noble example they have shown to us, and the food for future thought and work they have accumulated.[2]

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[1] See a paper by Professor Pickering, *Journal of the Franklin Institute*, December 1869, p. 373.

[2] Since writing the above, I find the following independent testimony in favour of Dr. Frankland's and my own notion of the corona in the *Astronomische Nachrichten*, from the pen of Dr. Gould. He says:— "Its form varied continually, and I obtained drawings for three epochs at intervals of one minute. It was very irregular in form, and in no

II. MY COMMUNICATION TO THE ROYAL SOCIETY.[1]

By the kindness of Professors Winlock, Morton, and Newton, I have been favoured with photographs, and as yet unpublished accounts, of the results of the recent total eclipse of the sun observed in America. I am anxious, therefore, to take the opportunity afforded by the subject being under discussion, to lay a few remarks thus early before the Royal Society.

Points to be elucidated.

The points which I hoped might be more especially elucidated by this eclipse were as follows:—

1. Is it possible to differentiate between the chromosphere and the corona?

2. What is the real photographic evidence of the structure of the base of the chromosphere in reference to Mr. W. De La Rue's enlarged photographs of the eclipse of 1860?

3. What is the amount of the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere?

4. Is there any cooler hydrogen above the prominences?

5. Can the spectroscope settle the nature of the corona during eclipses?

Point 1.

With regard to 1, the evidence is conclusive. The chromosphere, including a "radiance," as it has been termed by Dr. Gould (the edge of the radiance as photographed being in places strangely like the edge of the chromosphere viewed with the open slit), is not to be confounded with the corona.

On this subject, in a letter to Professor Morton, Dr. B. A. Gould writes:—"An examination of the beautiful

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apparent relation with the protuberances on the sun, or the position of the moon. Indeed, there were many phenomena which would almost lead to the belief that it was an atmospheric rather than a cosmical phenomenon. One of the beams was at least 30′ long." (1869.)

[1] Received December 7, 1869; printed in *Proc. R. S.* vol. xviii. p. 179.

photographs made at Burlington and Ottumwa by the sections of your party in charge of Professors Mayer and Haines, and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the moon in the pictures made during totality is not

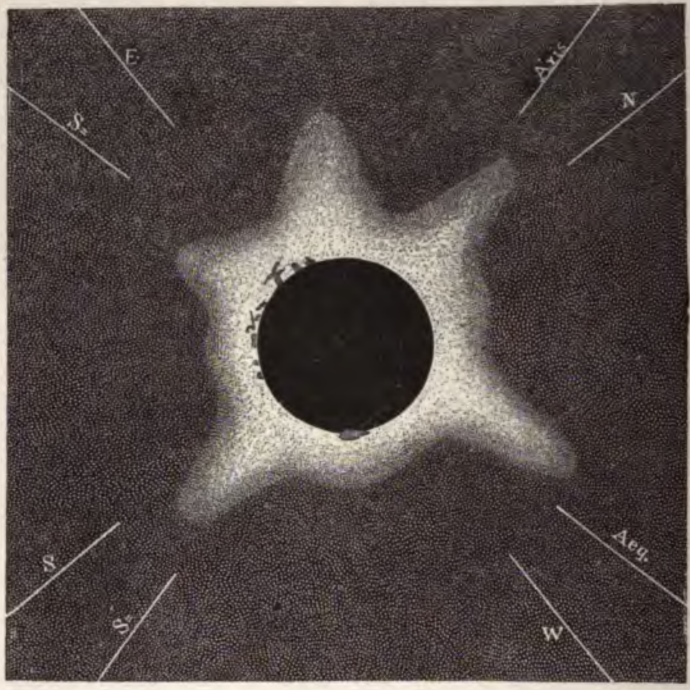

FIG. 105.—Gould's drawing of the corona, near the beginning of totality, at Burlington.

the corona at all, but is actually the image of what Lockyer has called the chromosphere.

"This interesting fact is indicated by many different considerations. The directions of maximum radiance do not coincide with those of the great beams of the corona; they remain constant, while the latter were variable. There is a diameter approximately corresponding to the solar

There is no relation between the coronal beams and the prominences.

axis, near the extremities of which the radiance upon the photographs is a minimum, whereas the coronal beams in these directions were especially marked during a great part of the total obscuration. The coronal beams stood in no apparent relation to the protuberances, whereas the aureole seen upon the photographs is most marked in their immediate vicinity; indeed the great protuberance, at 230° to 245°, seems to have formed a southern limit to the radiance on the western side, while a sharp northern limit is seen on all the photographs at about 350°, the intermediate arc being thickly studded with protuberances which the moon displayed at the close of totality. The exquisite

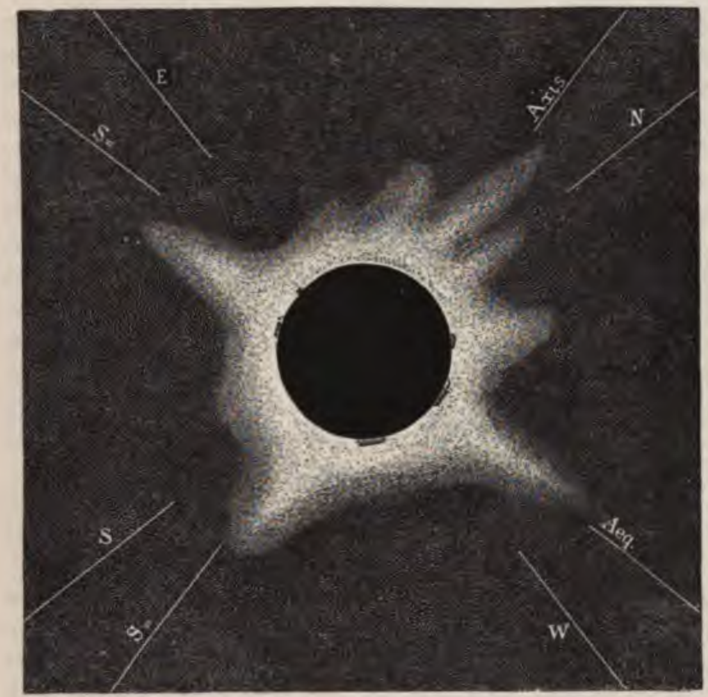

FIG. 106.—Gould's drawing of the corona, near the end of totality, at Burlington.

masses of flocculent light on the following limbs are upon the two sides of that curious prominence at 93°, which at first resembled an ear of corn, as you have said, but which, in the later pictures after it had been more occulted, and its southern branch thus rendered more conspicuous, was like a pair of antelope's horns, to which some observers compare it. Whatever of this aureole is shown upon the photographs was occulted or displayed by the lunar motion, precisely as the protuberances were. The variations in the form of the corona, on the other hand, did not seem to be dependent in any degree upon the moon's motion. The singular and elegant structural indication in the special aggregations of light on the eastern side may be of high value in guiding to a further knowledge of the chromosphere. They are manifest in all the photographs by your parties which I have seen, but are especially marked in those of shortest exposure, such as the first one at Ottumwa. In some of the later views they may be detected on the other side of the sun, though less distinct; but the very irregular and jagged outline of the chromosphere, as described by Janssen and Lockyer, is exhibited in perfection."

Point 2.

The second point is also referred to in the same letter. I think the American photographs afford evidence that certain appearances in parts of Mr. De La Rue's photographs, which represent the chromosphere as billowy on its under side, are really due to some action either of the moon's surface or of a possible rare lunar atmosphere; so that it is not desirable to confound these effects with others that might be due to a possible suspension of the chromosphere in a transparent atmosphere, if only a *section* of the chromosphere were photographed.

Dr. Gould's evidence.

Dr. Gould writes:—"You will observe that some of the brighter, petal-like flocculi of light have produced apparent indentations in the moon's limb at their base, like those at the bases of the protuberances. These indentations are evidently due to specular reflection from the moon's surface, as I stated to the American Association at Salem last month. Had any doubt existed in my mind previously, it would have been removed by an inspection of the photographs."

Where the chromosphere is so uniform a light that the actinic effect on the plate is pretty nearly equal, the base of the chromosphere is absolutely continuous in the American photographs; but in the case of some of the larger prominences, notably those at + 146 (Young) and — 130 (Young), there are strong apparent indents on the moon's limb.

Point 3.

I next come to the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere.

This is considerable; in fact the evidences of it are very much stronger than one could have wished, but hardly more decided than I had anticipated. Professor Winlock's evidence on this point, in a letter to myself, is as follows:—

"I examined the principal protuberance before, during, and after totality. I saw three lines (C, near D and F) before and after totality, and eleven during totality; *eight were instantly extinguished on the first appearance of sunlight.*"

This effect was observed with two flint prisms and seven inches' aperture. Professor Young, with five prisms of 45° and four inches aperture, found the same result in the part of the spectrum he was examining at the end of the totality.

Professor Young's evidence.

He writes: "I had just completed the measurements of 2602, when the totality ended. *This line disappeared instantly*, but 2796 [the hydrogen line near G] was nearly a minute in resuming its usual faintness." These observations I consider among the most important ones made during the eclipse; for they show most unmistakably that, as I have already reported to the Secretary of the Government-Grant Committee, the new method, to be employed under the best conditions, must be used with large apertures and large dispersion.

Point 4.

On the fourth point the evidence is but negative only, and therefore in favour of the view I have some time ago communicated to the Royal Society.

CHAP. XV.

Point 5.

We next come to the question of the corona,—a question which has been made more difficult than ever (in appearance only, I think) by the American observations.

I propose to discuss only the spectroscopic observations of Professors Young[1] and Pickering in connection with Dr. Gould's before-quoted remarks.

Professor Pickering, with an ordinary chemical spectroscope, merely directed to the sun's place during totality, obtained the combined spectrum of the protuberances and corona. He saw a continuous spectrum with two or three bright lines, one " near E," and a second " near C."

Professor Young, who used a spectroscope specially adapted for the work, in which only one part of the prominence at + 146° was being examined, saw C, near D, a line at 1250 ± 20, and another at 1350 ± 20 of Kirchhoff's scale. The rest of the observations I give in his own words :—

Extension of 1474 K, the green coronal line, across the whole width of the spectrum.

"Then came the 1474 K line, which was very bright, though by no means equal to C and D_3; but attention was immediately arrested by the fact that, unlike them, it extended clean across the spectrum; and on moving the slit away from the protuberances, it persisted, while D_3, visibly in the edge of the field, disappeared. Thus it was evident that this line[2] belonged not to the spectrum of the protuberance, but to that of the corona. My impression, but I do not feel at all sure of it, is that the two faint lines between it and D_3 behaved in the same manner, and are also corona lines.[3]

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[1] Professor Young's observations will be given *in extenso* in the Appendix.

[2] "On two or three occasions previously I had been very much surprised at not being able to detect this line in the spectrum of unusually bright prominences. On the other hand, I once found it very easy to see at a place on the sun's limb where the other chromosphere lines, usually far more brilliant, were almost invisible."

[3] "A careful examination of the photographs, especially No. 2 of the Burlington totality pictures, somewhat diminishes my confidence in the conclusion of the text as to the nature of these three lines (1250, 1350, and 1474). They certainly do not belong to the spectrum

Professor Pickering's observation.

"I am confirmed in this opinion by Professor Pickering's observation. He used a single-prism spectroscope, with the slit of the collimator simply directed to the sun, and having no lens in front of it. With this arrangement he saw only three or four bright lines, the brightest near E (1474). Now this is exactly what ought to occur if that line really belongs to the corona, which, from its great extent, furnished to his instrument a far greater quantity of light than the prominences.

Appearance of the F line.

"By this time the moon had advanced so far that it became necessary to shift the slit to the great prominence on the opposite side of the sun. While my assistant was doing this, I suppose I must, in the excitement of the moment, have run my eye-piece over the region of the magnesium lines (*b*), and thrown them out of the field before he had brought anything upon the slit. At any rate I saw nothing of these lines, which were evident enough to several other observers, and can think of no other way to account for their having escaped me. The F line in the spectrum of the great protuberance was absolutely glorious, broad at the base and tapering upwards, crookedly, as Lockyer has before often observed. Next appeared a new line, about as bright as 1474 at 2602 ± 2 of Kirchhoff's

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of the most brilliant portion of the prominences; but around the prominences of the eastern limb, on which the slit of the spectroscope was directed during the first half of the totality, the photograph shows a pretty extensive and well-defined nebulosity, evidently distinct from, though associated with, the brilliant nuclei. Now it is possible that these lines may belong to this nebulosity, and not to the corona proper; for I cannot recall with certainty whether 1474 retained its brilliance at any considerable distance from the prominences, or only in their immediate neighbourhood. My strong impression, however, is that the former was the case, and that the text is correct. I may as well confess that my uncertain memory here is due to the fact that just at this time, while my assistant was handing me the lantern with which to read the micrometer-head, I looked over my shoulder for an instant, and beheld the most beautiful and impressive spectacle upon which my eyes have ever rested. It could not have been for five seconds: but the effect was so overwhelming as to drive away all certain recollection of what I had just seen. What I have recorded I recall from my notes taken down by my assistant."

scale. Its position was carefully determined by micrometrical reference to the next line, 2796 K (hydrogen γ), which was very bright; *h* was also seen, very clear, but hardly brilliant. In all, I saw nine bright lines.

"A faint continuous spectrum, without any traces of dark lines in it, was also visible, evidently due to the corona. Its light, tested by a tourmaline applied next to the eye, proved to be very strongly polarized in a plane passing through the centre of the sun. I am not sure, however, but that this polarization, as suggested by Professor Pickering, may have been produced by the successive refractions through the prisms. This explanation at once removes the difficulty otherwise arising from the absence of dark lines."

The light of the corona polarized.

I have first to do with the continuous spectrum, deduced from Professor Pickering's observations.

I think in such a method of observation, even if the corona were terrestrial and gave a dark line spectrum, the lines visible with such a dim light would in great part be obliterated by the corresponding bright lines given out by the long arc of chromosphere visible, to say nothing of the prominences, in which it would be strange if C, D, E, *b*, F, and many other lines were not reversed. This suggestion, I think, is strengthened by the statement that two bright lines were seen "near C" and "near E;" should we not rather read (for the "near" shows that we are only dealing with approximations) C and F, which is exactly what we might expect?

But even this is not all that may be hazarded on the subject of the continuous spectrum, which was also seen by Professor Young under different conditions.

Assuming the corona[1] to be an atmospheric effect merely, as I have before asserted it to be, it seems to me that its spectrum should be continuous, or nearly so; for is it not as much due to the light of the prominences as to the light of the photosphere, which, it may be said roughly, are complementary to each other?

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[1] See note on page 243.

With regard to the aurora theory, I gather from Professor Young's note that, if not already withdrawn, he is anxious to wait till the next eclipse for further facts. I consider that the fact that I often see the line at 1474, and often do not, is fatal to it, as it should be constantly visible on the proposed hypothesis. The observation of iron-vapour, as I hold it to be at this elevation, is of extreme value, coupled with its simple spectrum, *seen during an eclipse*, as it entirely confirms my observations made at a lower level in the case, not only of iron but of magnesium.

[The following communication from Professor Young, printed in *Nature*, March 24, 1870, commenting upon some of the views advanced above, is here given:—

"It is not impossible that the so-called corona may be complex. Some portion of its radiance may, *perhaps*, originate in our own atmosphere, although I do not yet find myself able to accord with the conclusions of Dr. Gould and Mr. Lockyer in this respect, and am strongly disposed to believe that the *whole* phenomenon is purely solar.

"This much appears certain, however, that there exists outside of the chromosphere properly so called (*i.e.*, the envelope of *rea hydrogen*), and as distinct from *it* as it is from the *photosphere*, an immense atmosphere of self-luminous substance, extending to a distance of from 5′ to 8′ from the sun's surface, and probably much further in places—phosphorescent dust or fog in a glowing gas.

"In support of this idea I adduce the photograph of Mr. Whipple, taken at Shelbyville, Ky., with an exposure of 40ˢ. On this, the *photolytic corona* (if I may use the expression to distinguish it from the *visible corona*, whose points of maximum brilliance were, according to Dr. Gould, entirely different) reaches a height of 6′. Professor Harkness observed the 1474 line in the spectrum of the corona at a distance of nearly 5′ from the sun's limb, and not near to any prominence. I do not know the precise elevation at which I saw it, but it was not less than 3′ or 4′.

"Indirectly, also, the idea is confirmed by the spectroscopic observation of Professor Pickering, who used a single prism instrument, with the slit simply directed towards the sun, not attached to a telescope. He saw only three or four lines, the brightest in the green near E. Now, since this line, when observed by throwing a large image of the sun on the slit, is very faint as compared with C, D_3, and F, its intensity, as seen by him, can only be accounted for by supposing that the luminous area from which it was derived far exceeded that of the chromosphere and prominences.

"I have noticed also that some of the observers of the Indian eclipse (Rayet and Pogson) speak of the intensity of the green line. Did they observe in the same manner as Professor Pickering?

"I need hardly add that Professor Pickering's observation of the non-polarization of the corona concurs with what has been said.

"As to the faint continuous spectrum, I am sure that the reported absence of dark lines was not the result of insufficient observation.

"I could not have failed to see D, E, *b*, 1961, F and G, had they existed; for in a spectrum of similar brightness formed by a light from a cloud, not only these but many other lines are visible in my instrument. Now, the absence of some of these might, perhaps, be accounted for on the ingenious hypothesis proposed by Mr. Lockyer; but this would not apply to D, E, or G. [Why not?—J. N. L.]

"But if we admit the existence of faintly luminous solid or foggy matter near the sun, either meteoric or arising from the cloudy condensation of a non-permanent gas, the whole is at once easy of comprehension."

The auroral theory of the corona is thus referred to:—

"The objection pressed by Mr. Lockyer that the bright line 1474 is only occasionally visible, is, I think, unfounded. At any rate I have never failed to see it myself when looked for, and very seldom to make it visible to others when I have wished to exhibit to them. It is faint, and, like a difficult microscopic object, requires management to bring it out with five prisms; but by placing the slit tangential to the sun's disc, and giving the instrument a slight jar, it is seen to flash out as the limb passes off the slit. It is worth noting, too, that it is often especially plain at portions of the limb where the chromosphere is unusually shallow and faint.

"But while I think it probable that this line coincides with the aurora line reported by Professor Winlock at 1550 of Mr. Huggins' scale, I am by no means sure of it. I understand its assigned position rested upon a single observation with a chemical spectroscope, and the probable error of such a determination cannot well be less than ten divisions of Kirchhoff's scale. I have naturally made many attempts to determine its position for myself, but have never seen it except thrice, and then not long enough at a time to complete a measurement. I am only sure that its position lies between 1460 and 1490 of Kirchhoff.

"For this reason, although I do not at all abandon the hypothesis, which appears to have other elements of probability in the general appearance of the corona, the necessity of intense electrical disturbances in the solar atmosphere as the result of the powerful vertical currents known to exist there, as well as the curious responsiveness of our terrestrial magnets to solar storms; yet I do not feel in a position to urge it strongly, but rather await developments.

"As to the substance which causes this line, I observe that Father Secchi, in a recent communication to the French Academy, is disposed to think it hydrogen; while Mr. Lockyer still believes it to be

Professor Young's letter.

iron. I am in hopes that experiments now in progress may throw some light on the subject.

"May I suggest, in closing this long communication, that it seems to me that valuable observations might be made at the Eclipse of next December, by fitting up telescopes with a ground glass sliding screen, upon which an image of the corona two or three inches in diameter should be thrown; the ground glass having the roughened side next the observer, so that he could sketch upon it with a lead pencil the outlines of the image, the glass being made long enough to allow of several such sketches.

"The comparison of a series of such outlines would decide the question of changes in the coronal streamers, as the sketches, being simple tracings, could not but be accurate in their indications of position."]

THE MEDITERRANEAN ECLIPSE, 1870.[1]

I.—A LETTER FROM VENICE.[2]

CLOUD in Sicily, cloud in Spain, cloud in Africa. Such at first sight might seem to be the only result of all the observations made on the eclipsed sun of 1870; such the reception given by Nature to those who wooed her as she

[1] I should be wanting in gratitude if I did not record one fact here in connection with this expedition. Owing to those whose duty it was to bring the requirements of science before the Government appealing to the wrong department, and other causes, funds to aid the observations were in the first instance refused. At this juncture, Professor Peirce, the distinguished director of the expedition equipped in the most liberal manner by the American Government, arrived in this country, and hearing of the position of affairs, at once did me the honour to send me the following letter:—

"FENTON'S HOTEL.

Professor Peirce's letter.

"MY DEAR SIR,—I have been directed by the Government of the United States to have the best possible observations made of the total eclipse of next December. If I could aid the cause of astronomy by assisting the observers of England in their investigations of this phenomenon, I should be greatly pleased. I take the liberty therefore to invite your attendance, and also that of other eminent physicists of England, with either of the parties of my expedition, one of which will go to Spain and the other to Sicily.

"Yours very respectfully and faithfully,

"BENJAMIN PEIRCE.

"J. NORMAN LOCKYER, Esq. F.R.S."

I at once accepted this generous offer.

I must also add, in justice to our own Government, that after some of my instruments had been shipped to Sicily another application was made, and it fell to my lot by an accidental circumstance to be called had never been wooed before, who approached her full of the rarest gifts which Science has placed at man's disposal.

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[2] Where I was snowed up for two days!!

But, after all, has the oracle been silent? I think not. Dare we, however, say that the great problem of the Corona, that one among the many still outstanding difficulties which the eclipse was invoked to settle, is settled? This, perhaps, would be saying too much, but still, I think, a step in advance has been made. The oracle has spoken darkly perhaps, but it *has* spoken.

upon to state the requirements of science to the Chancellor of the Exchequer Mr. Lowe, and Mr. Stansfeld, who thus heard of the matter for the first time. Nothing could exceed the anxiety at once shown to aid the work; and the subsequent supply of ships, money, and general aid left nothing to be desired.

The expedition which eventually left England was made up as follows:—

A.—SPANISH AND ALGERIAN PARTY.

I. Cadiz detachment. In charge—The Rev. S. J. Perry.

Spectroscope—The Rev. S. J. Perry and assistant (Mr. Hostage), Mr. Abbay.

Polariscope—Mr. Moulton, Mr. Hudson, Mr. Fison.

Sketches of Corona—Mr. Naftel, Mr. Smyth, Mr. Penrose, Mr. Collins.——*Time and General Observations*—Captain Toynbee.

II. Gibraltar detachment. In charge—Capt. Parsons.

Spectroscope—Mr. Carpmael, Mr. Gordon.
Polariscope—Mr. Lewis, Mr. Ladd, Mr. Baynes.
Photography—Mr. Buckingham and assistant.
Sketches of Corona—Mr. Hunter, Mr. Anson, Mr. Harrison.
Saturn in the Corona—Mr. Talmage, Mr. Maclear.

III. Oran detachment. In charge—Mr. Huggins.

Mr. Huggins, Admiral Ommanney, Lieutenant M. F. Ommanney, Professor Tyndall, Rev. F. Howlett, Mr. Carpenter, Mr. Crookes, Captain Noble, Dr. Gladstone.

B.—SICILIAN PARTY.

In charge—Mr. Lockyer.

Spectroscope—Mr. Lockyer and assistant (Mrs. Lockyer), Professor Roscoe and assistant (Mr. Bowen), Mr. Seabrooke, Mr. Pedler, Mr. Burton.

Polariscope—Mr. Ranyard, Mr. Griffith, Mr. Clifford, Mr. Harris, Professor Adams.

Sketches of Corona—Mr. Brett, Mr. Darwin.

Photography—Mr. Brothers, Herr Vogel, Mr. Fryer.

Time and General Observations—Mr. Vignoles, sen., Mr. Vignoles, jun.——*Chemical Intensity*—Professor Thorpe.

Let me endeavour to put the question as it stood a few weeks ago as briefly as possible.

Beginning the story some few years back, we find the corona, a halo of white light round the moon, with a height sometimes represented as equal to the moon's diameter, sometimes more, sometimes less, with a border *à discrétion*,—so much did the drawings vary—regarded as the solar atmosphere.

Some thought the red prominences to be mountains, other observers called them clouds.

The polariscope used.

The polariscope was brought up with a view of determining whether the corona shone by reflected light or not. The result of this new method of observation was doubtful.

In the Indian eclipse of 1868 M. Janssen, by means of the spectroscope, still another aid, determined that the prominences were masses of hydrogen gas, but there was no final word about the corona. Major Tennant observed that its spectrum was continuous. Later in the same year Dr. Frankland and myself approximately determined the pressure of the prominence gases by means of the new method and laboratory experiments, and at once stated our conviction that the extensive corona which had been depicted and represented by Kirchhoff and others to be the solar atmosphere must be something else. This was our idea. I cannot quote our words, for I am writing in Venice and have no copies of our paper with me.

The American eclipse.

In the American Eclipse of 1869 the problem was advanced considerably, perhaps even more considerably than we can yet form an idea of, writing as we must still do doubtfully. I do not refer to the drawings, for they varied considerably, but to the observation that the light of the outer corona, like that of the prominences, gave a bright-line spectrum. But as at least some of the observers gave positions doubtfully, "near C" and "near E," I thought that the explanation was still possible which regarded the corona as of terrestrial origin; that is, which assumed it to be an appearance due to the presence of light in our own atmosphere. The problem was one of such difficulty that there seemed a possibility that, by some unexplained cause, some of the solar light might be diffused and beat out of its course, and then, mixing up with the light of the chromosphere, give us a sort of continuous spectrum, with the hydrogen bright lines superposed upon it; in other words, that as the eye perceives a bright, irregular region or glare around the uneclipsed sun, an effect due to our atmosphere, so also the eye might perceive a bright, irregular region or glare round the *uneclipsed chromosphere during eclipses*, due also to our atmosphere.

One word here about the Chromosphere, the name I have given to the bright-line-giving region outside the photosphere. It has long been clear that the spectroscopic method of observing it when the sun is not eclipsed is not totally effective; that is to say, that we only see a percentage of it—perhaps only a relatively small percentage—but the glowing prominences, that is, those in which there is no evidence of the rapid motion of ejection from the sun, the ejection taking place at all angles from the line of sight, afford evidence that there is probably a layer of cooler hydrogen susceptible of being rendered visible above the ordinary level. Now as these prominences may be 5′ high, it is not unreasonable to suppose that the chromosphere may even extend to that distance, or even a little beyond it.[1]

Our atmosphere partly obliterates the chromosphere.

[1] Here is what I wrote on this point a year ago:—"I next come to the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere. This is considerable; in fact the evidences of it are very much stronger than one could have wished, but hardly more decided than I had anticipated. Professor Winlock's evidence on this point, in a letter to myself, is as follows:—'I examined the principal protuberance before, during, and after totality. I saw three lines (C, near D, and F) before and after totality, and eleven during totality; *eight were instantly extinguished on the first appearance of sunlight.*' This effect was observed with two flint prisms and 7 inches' aperture. Professor Young, with five prisms of 45° and 4 inches' aperture, found the same result in the part of the spectrum he

Hence it was that in the Instructions to Observers,[1] drawn up by Professor Stokes and myself, and approved by the Organizing Committee for this 1870 Eclipse, it is stated that—

Objects of the expedition of 1870.

"The PRINCIPAL OBJECT to be obtained is to determine whether it is possible to differentiate the outer layers of irregular outline and the streamers (of the corona) from a stratum, say some 5′ or 6′ high, round the sun, which may possibly be the limit of the gaseous envelopes above the photosphere."

The spectroscopic observers, therefore, were enjoined—

(*a*) "To determine the actual height of the chromosphere as seen with an eclipsed sun: that is, when the atmospheric illumination, the effect of which is doubtless only partially got rid of by the Janssen-Lockyer method, is removed. If the method were totally effective, the C line, the line of high temperature, should hardly increase in height; but there can be little doubt that the method is not totally effective, so the increase in height should be carefully noted."

(*b*) "To determine if there exists cooler hydrogen above and around the vividly incandescent layers and prominences."

And the polarizers

"To examine a detached and selected part of the corona about 6′ from the limb of the sun, and say about 8′ in diameter."

Having got so far, it may be here stated that of the three means of attack, namely, the spectroscope, the

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was examining at the end of the totality. He writes:—'I had just completed the measurements of 2602, when the totality ended. *This line disappeared instantly*, but 2796 (the hydrogen line near G) was nearly a minute in resuming its usual faintness.' These observations I consider among the most important ones made during the eclipse; for they show most unmistakeably that, as I have already reported to the Secretary of the Government-Grant Committee, the new method to be employed under the best conditions must be used with large apertures and large dispersion."—*Proc. R. S.*, 1870, p. 181.

[1] These will be found in the Appendix.

polariscope and telescope, and naked-eye observations, the spectroscopic method, under certain circumstances, might have been by far the most doubtful, the polariscope method coming next.

Meaning of the spectroscopic indications.

With regard to the spectroscopic observations, if we assume that no light whatever is received by and from our own atmosphere, the observations would be easily translated. A pure continuous spectrum would reveal to us solid or liquid matter in the circumsolar regions; a spectrum continuous or not containing bright lines would give us gases or vapours; the ordinary solar spectrum, with its dark lines, would indicate matter incapable of radiation itself, and therefore cool, reflecting to us ordinary sunlight. It is clear that the problem would be complicated if circumsolar matter both reflected sun-light and sent us its own; and still more so if we allow that the coronal light may be partly contributed from reflections and refractions in our own atmosphere. Then we have to consider whether the light thus contributed may possibly be due to the photosphere or to the prominences, and we are landed in a maze of difficulties which need not be discussed here.

Use of sketches made at different places.

The system of sketching introduced for this eclipse is at once so simple and final that the only wonder is it has not been introduced before. The corona must be either solar, atmospheric, or subjective, that is, more or less built up in the observer's eye, this more or less depending *cæteris paribus* upon the brilliancy of the undoubted solar portion. If at all stations, the stations being as wide apart as they have been this time, the drawings are similar, then the corona would be undoubtedly cosmical; if dissimilar, then it would either be terrestrial or subjective: and this point could and would have been settled this time, if the weather had permitted, by arranging the observers *in pairs*, —that is, dealing with two observers in each place instead of a single one, and so obtaining the eye-variation.

This being premised, what is the result of the very few observations, comparatively speaking, which have been made? Before I attempt to give any idea of my answer to this question, it is only fair to myself to state that my only sources of information, up to the present time, have been conversations with some of the American members of the Sicilian expedition, a brief telegram from the members of the English party at Agosta, the Rev. S. J. Perry's communication to the *Daily News* of the 2nd instant, and an inspection of some drawings made by the officers of H.M. ships off Aci Reale. At Catania we saw a portion of the corona for 1½ seconds through a cloud, and that was all; and the day after the eclipse, before the more fortunate members of my party returned, it became my duty to proceed to Malta in H.M.S. *Lord Warden* to attend the court-martial on the officers and crew of the beautiful, but unfortunate *Psyche*, in which we had been wrecked on the 15th ult., and the weather in the Mediterranean has been so bad that it was impossible to leave Malta in time to rejoin the expedition before they left for England. Of detailed information, therefore, I have none.

What the corona was like in the drawings.

In the first place, then, I submit that the fact that the corona is a compound phenomenon comes out in an unmistakable way. We have first of all a ring some 5′ or 6′ high round the moon, which almost all observers have seen alike; and then we have light beyond which some observers have seen of one shape and some of another, now stellate with many rays, now stellate with few, now absolutely at rest, now revolving rapidly.

This I think to be the key-note of all the observations with which I have become acquainted. I need scarcely say that it is exactly what had been predicted.

First among the fortunate ones who observed the corona with the telescope was Prof. Watson, of Ann Arbor, who took up his station at Carlentini, and appears to have been the best favoured among the Sicilian observers. From his account I gather that there was an almost perfect *shell* around the sun about 5′ high, and that outside this shell were less definite rays. What he was particularly struck with was this, that, as seen in the telescope, the rayed portion was most developed over the prominences, and, as I gathered from him in one case, the rayed portion was absent as if a veil had been removed; so that he, at all events, is strongly impressed with the idea that the shell represented a true solar appendage, and that the rayed structure was due to our own atmosphere.

Telescopic appearance of the corona.

Next comes Mr. Brett, who, although he was not so fortunate, still was enabled to see and place on record some most interesting features, including the whole outline of the corona and even some of the protuberances. He also, as I am informed, saw the rayed portion of the corona most developed above the protuberances, the outline of the interior portion being visible, though not so strongly marked as in the case of Prof. Watson's drawing, in consequence of less favourable atmospheric conditions. I am thankful to say that the weather at Syracuse enabled Mr. Brothers to obtain some admirable photographs, which I have not yet seen. These are among the most important results of the Expedition.

Next I must mention Prof. Peirce, the head of one of the American parties, who observed two miles north of Catania, at a private casino of the Marchese Sangiuliano. I believe that he also saw the shell, but of this I am not absolutely certain; but he distinctly observed that the outer corona over the prominences was rosy red, although he did not see the prominences himself. A more beauful proof of the terrestrial nature of this portion of the corona it would be difficult to imagine; for, of course, at the sun, the hydrogen, which thus tinged it, is incapable of colouring anything, as its own light is absorbed by the transcendent brilliancy of the photosphere; while nothing would be more natural than to suppose that the light, which in its own atmosphere should strongly tinge anything radially illuminated, should be that of the prominences.

But the strongest proof of the variability of the outer portion and of the constancy of the inner portion is afforded by the observations made on board the small fleet attempting to save the *Psyche* off Aci Reale, where the eclipse was observed in unclouded splendour. Here were the ironclads *Lord Warden*, *Caledonia*, and *Royal Oak*, and the tugs *Weasel* and *Hearty*, besides the Italian gunboat *Plebiscito*, all within a stone's throw of each other. In all the drawings, and many have been received, we have a ring 5′ or thereabouts, while the outer portion is as variable as may be. On the same deck, that namely of the flag-ship *Lord Warden*, two drawings were made, one by Capt. Brandreth, and the other by Dr. Macdonald, F.R.S., in which the variation is so strong that one would feel inclined to acquit the atmosphere of any participation in the matter, and to relegate the whole outer corona to subjectivity alone, did not Mr. Brothers' admirable photographs show both phenomena, as I am told they do. Dr. Macdonald saw eight rays arranged with perfect symmetry; Captain Brandreth saw only two elliptical hoops crossing each other at right angles.

All the drawings made on board the men-of-war at Aci Reale were different.

Captain Cochrane of the *Caledonia*, besides the ring, saw a complicated stellate figure, the rays of nearly equal length; while Mr. Dexter, at sea between Catania and Syracuse, saw, besides the ring, *only one ray* of inordinate length.

So much for the drawings. I think that if the records of former eclipses be now examined, especially Mr. Carrington's drawing of the eclipse of 1851, and compared with the others taken at the same time, additional evidence will be gathered in favour of the compound nature of the corona, which, on the evidence now before me, I consider the great teaching of the present eclipse. Our experience in Sicily seems to be similar to that of the Spanish observers, for Mr. Perry writes that "some observed two curved rays," while the rapid degradation of light occurred at one-fifth of a solar diameter, but, so far as I know, no one in Sicily was favoured with a view of the dark intervals which were observed in Spain.

The green coronal line seen.

There is a strange and most interesting discordance between some of the spectroscopic observations made in Sicily and Spain. At Agosta, where the totality was well visible for ten seconds, Mr. Burton detected a green line near E, with a tangential slit (distance from moon not stated). This line, which was also seen by the Italian observers, is doubtless the one recorded last year by the American astronomers, but in Spain Mr. Perry states that bright lines at C near D, *b* (or E) and F were observed 8′ away from the sun. At Syracuse Prof. Harkness, whose telescope was moved into the various positions by Captain Tupman, R.M.A., found the green line in all parts of the corona, so far as about 10′ from the sun, and at one point thought he detected two green lines, less refrangible than it; but at several places he saw a complete hydrogen spectrum (including C), which he attributed to prominences, until he was informed by Captain Tupman that there was no prominence near the slit. More proofs of the terrestrial nature of this portion of the corona, I think, taken in connection with the fact that *the dark moon gave identically the same spectrum*. It would appear that there was so much atmospheric reflection in Spain, and here and there at Syracuse, that the true coronal spectrum with its line near E, the existence of which we must now accept as established beyond all question, was partially masked by the prominence spectrum with its usual well-known lines. There is one passage in Mr. Perry's interesting letter in which, if there be a misprint, as I suspect there is, an observation of great importance is recorded. It runs, "Mr. Abbay, observing at Xeres with a spectroscope of 2 prisms of 45° belonging to Professor Young, saw the bright lines C, D, F; and afterwards F and a line rather more bright than F on the less refrangible side of B, C not noticed then." Now, if *b* (not B) was intended here we have sub-incandescent hydrogen mixed with the green-line-giving

The dark moon gave the same spectrum as a portion of the corona.

substance, which may probably be a new element with a vapour density less than hydrogen.

So that roughly we might regard the chromosphere to be built up of the following layers, which are in the orders of vapour density in the case of known elements:—

Substances in the corona.

X′ (new element)		Green coronal line.
Hydrogen	Sub-incandescent	F.
	Incandescent	C, F, near G, h.
X (new element)		Near D.
Magnesium		*b* and lines in blue and violet.
Sodium		D
Barium		Several lines.
Iron, &c.		Several lines, including E.

The foregoing table excludes naturally the substance or substances which give bright lines in the solar spectrum, which are at times visible in the spectrum of the chromosphere. I have ventured to suggest that the substance which gives the line in the green is a new element, because invariably I have found that in solar storms the chromospheric layers are thrown up in the order of vapour density, and because all the heavier vapours are at or below the level of the photosphere itself.

The polarization of the corona.

With regard to the question of polarization, the parties in Sicily obtained evidence that the corona was radially polarized, though Professors Harkness and Eastman obtained a result which they explain differently. Mr. Ranyard, at Villamonda, and Mr. Peirce, jun., north of Catania, obtained identical results in favour of strong polarization. Hence the solar corona, accepting these observations, not only radiates, but reflects solar light to us. A careful consideration of this fact, taken in connection with the possible addition of a, so to speak, terrestrial corona to its light, may enable us to account for some of the observations, both polariscopic and spectroscopic, which do not at first appear to harmonize with those to which I have referred, notably those which give a pure continuous spectrum to the corona, and which state that its light is only slightly polarized.

From what has preceded, then, we seem justified in suggesting as working hypotheses the following, which, however, more accurate information may alter, and which I offer as suggestions only, *bien entendu*.

The working hypotheses resulting from the observations.

1. The Solar Chromosphere extends some 5′ or 6′ from the sun (Watson and others), its last layers consisting of cool hydrogen (Mr. Abbay), and possibly a new element with a green line in its spectrum (Young, Burton, and others); which line, if it be identical with the auroral line as stated by Gould, may possibly be present in the higher regions of our own atmosphere.

2. Outside this stratum the rays, &c., are for the most part due partly to our own atmosphere, partly to our eyes, for their shape varies; they are seen by some at rest, by others in motion, and their spectrum is the same as that of the dark moon (Maclear).

3. The white light of the chromosphere above the prominences, as seen in an eclipse, is due to its strong reflection of solar light, as shown by the polariscopic observations (Ranyard, Peirce, jun., Ladd).

4. The rosy tinge of the corona proper, that is of the region more than 5′ or 6′ from the sun, is due to our atmosphere containing light which comes from both the higher and lower strata of the chromosphere (Peirce, sen., Maclear, Abbay).

VENICE, *Jan.* 9, 1871.

THE MEDITERRANEAN ECLIPSE, 1870

(*continued*).

II.—MORE LIGHT.

CH. XVII.

IN my former letter[1] under the above title, written from Venice, I gave as shortly as I could the conclusions at which I had arrived as to the results of the various Eclipse expeditions, as gathered from the very imperfect information then at my disposal. Since I returned home I have naturally become possessed of more facts, though even yet the time has not arrived for discussing all the observations, as they must be discussed before an absolutely final verdict can be given.

Still, there is so much general interest taken in the recent work, that I venture to return to it at the present time, more especially as I can now print a letter from a distinguished American astronomer, giving his view of the work done, and also as I am anxious to refer to Professor Young's article which has recently appeared in *Nature*.[2]

Professor Peters's letter.

Professor Peters, whose long and laborious researches on the sun are well known to all of us, thus writes in reference to my former article:—

"Its perusal has been to me a source not only of pleasure but of much instruction. You have placed on record, with

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[1] Reproduced in the last chapter.

[2] This will be found in the Appendix.

Professor Peters's letter.

great lucidity, the question as it stood before the Eclipse, and the points to be examined by the various ways of observation for bringing the question nearer to its solution. Although the unfavourable state of the weather over the entire zone of totality, as it seems, from Spain to Sicily, has greatly obstructed the execution of the plans and the extensive preparations made with the liberal aid of our respective governments; and although hitherto, of course, only imperfect, mostly verbal, information has reached us of what the parties really did succeed in obtaining—still the result that is to be drawn from the sum total, as you are showing, seems of importance. The spectroscopic, polariscopic, and telescopic observations altogether agree in demonstrating an interior portion of the corona to belong to the sun. The existence of such a *solar* stratum is sustained also by my researches on the motion of spots when near the limb, pointing to a refraction on, or rather above, the sun's surface. I concur further in your opinion that the outer, more irregular radiating portion of the corona very likely owes its origin to our atmosphere. It is highly to be regretted that our Etna parties, in elevations respectively of 3,100, 5,500, and 8,000 feet, suffered disappointment from a heavy cloud at the critical moment of totality. Their observations would have been decisive as to the local and atmospheric cause of the radiating coronal phenomenon."

One more extract before I proceed. With reference to the suggestion (based on my observations of injections into the chromosphere) contained in my article, that probably the green line seen in the spectrum of the corona might indicate a new element lighter than hydrogen, Professor Young, claiming priority in the suggestion, writes:—

What does the line 1474 represent?

"In *Silliman's Journal*, Nov. 1869, I wrote: 'Should it turn out that this line in the spectrum of the aurora does actually coincide with 1474, it will be of interest to inquire whether we are to admit the presence of *iron vapour* in and above our atmosphere, or whether in the spectrum of iron this line owes its origin to some foreign substance, probably some occluded gas as yet unknown, and perhaps standing in relation to the magnetic powers of that metal.'

"This is the only reference I am able to make here. In my paper published in the Proceedings of the American Association for 1869, the same thing is, I think, more forcibly expressed. I think you will also find it in my Eclipse Report in the 'Journal of the Franklin Institute' (and in my letter to *Nature* last spring).

"The idea that 1474 might represent some new element occurred to me at once when I found it in the corona, but of late I own I have more inclined to the opinion that it might possibly be a true iron line, and caused by meteoric iron dust of almost infinitesimal fineness; yet I have always felt the difficulty of supposing the complicated iron spectrum reducible to this one line."[1]

Professor Young's letter.

I feel it due to Professor Young to give this extract, though I confess I do not see that the suggestions are similar, nor do I see anything similar in the letter referred to, though I have lighted upon this passage which I had forgotten, which shows the great advance that has been made. Professor Young last year wrote:[2] "It is not impossible that the so-called corona may be complex. Some portion of its radiance may, *perhaps*, originate in our own atmosphere, although I do not yet find myself able to accord with the conclusions of Dr. Gould and Mr. Lockyer in this respect, and am strongly disposed to believe that the whole phenomenon is purely solar." His present views were given three weeks ago, as in the main concurring with my own.

With reference to Professor Young's article, I am anxious to say one word on the "sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality," witnessed by himself and Mr. Pye. I have seen this *once*, and only once, during all my observations, and Professor Young (who enjoys better atmospheric conditions than I do) has never seen it when working with the new method. Now, I hold that the new method is competent to pick up such an envelope as the one referred to by Mr. Longley, if it can pick up an uprush similarly composed; and although, of course, the vapours competent to give such lines ***are not far off, as the ordinary observations prove, I do not think they are ordinarily high enough above the level of the photosphere to be seen in this manner.***[1] That the number of lines is largely increased when the atmospheric glare is withdrawn, was proved during the American Eclipse.

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[1] It will be abundantly shown in the sequel from Professor Young's and my own researches that this line is *not* due to iron.

[2] *Ante*, p. 255.

The photographic record of the corona.

But to return to the corona, the main point of attack during the last Eclipse. Since my last article was written I have had an opportunity of inspecting copies of the beautiful photographs taken by Mr. Brothers at Syracuse, and also one of the photographs taken by the Americans in Spain. These, compared with the sketches taken at the respective stations, are very curious. In the Spanish photograph there is a very distinct "rift," or dark space in the coronal region, extending, I believe, almost to the sun, and fainter indications of two other such rifts in another region, not extending so low down in the corona. So far as the facts have yet been before me, only one of these rifts was sketched. Now, at Syracuse Mr. Brothers also photographed rifts—three rifts; but the sketches did not record a single one. In Professor Watson's drawing, a copy of which I have now in my possession, there is no indication whatever of them. But there is a much more important fact behind. Of course, if these rifts had been in the same positions in the two photographs, taken at stations so wide apart as Spain and Sicily, the presumptive evidence in favour of the solar nature of the corona for a distance outside the sun equal to its diameter, would have been overwhelming; and feeling that here was a crucial test to apply to a question which has so long been debated, but never with such interest among the workers as recently, it was with some excitement that I found myself before these two photographs some little time ago with two American astronomers of eminence, for the purpose of endeavouring to settle the question. Suffice it to say that we came to the conclusion that the rifts were not identical, that the two cameras had *not* photographed the same phenomenon, although at first there appeared to be sufficient similarity to make the matter appear doubtful, and, unfortunately, the photographs vary so much in size, and the margin of the American one is so limited, that it will be scarcely possible to make a final comparison until they are brought to a common scale, and superposed the one on the other. I do not think it is surprising that rifts should appear in both photographs, supposing a non-solar cause were at work, for the corona between the rifts on Mr. Brothers' photograph looks like a very wide ray.

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[1] This has been italicized in 1873. See the chapters on the Indian Eclipse and Part II.

Assuming, then, for the present that the photographic evidence goes the way of all the other evidence—that, in short, the solar corona, including all its fantastic boundaries, has been probably reduced from one, two, or three solar diameters, to six, eight, or ten minutes,—I care not which,[1]—let us examine some of the details of the various observations.

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[1] I beg here to give the actual words employed by Dr. Frankland and myself in the communication to the Royal Society on the subject. Speaking of the chromosphere, it was remarked, "The tenuity of this incandescent atmosphere is such that it is extremely improbable that *any extensive atmosphere*, such as the corona has been imagined to indicate, lies outside it."—*Proc. R. S.*, February 11, 1869. I never imagined that all the corona was non-solar. Again (*Proc. R. S.*, No. 116, 1870), discussing the American Eclipse, I state that the chromosphere includes the "radiance" observed in the American Eclipse, of which radiance Dr. Gould wrote as follows:—"An examination of the beautiful photographs made at Burlington and Ottumwa . . . and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the moon in the pictures made

In Professor Watson's drawing, the intimate connection between the higher and lower levels of the chromosphere (including the portions not at present observed by the new method) comes out in a very striking way. Mr. Seabrooke, at my request, made careful maps of the positions of the prominences before the totality commenced, and Professor Watson made his drawing of the corona independently of the positions of the prominences. On the homeward journey the map was compared with the sketch, and to use Professor Roscoe's words, "On comparing the two drawings thus independently made, a most interesting series of coincidences presented themselves. Wherever on the solar disc a large group of prominences was seen in Mr. Seabrooke's map, there a corresponding bulging out of the corona was chronicled on Professor Watson's drawing; and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the sun's limb." We may remark that these coincidences show the excessive fidelity of the

Coincidence of the bulging portions of the corona with the prominences.

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during totality is not the corona at all, but is actually the image of what Lockyer has called the chromosphere. This interesting fact is indicated by many different considerations. The directions of maximum radiance do not coincide with those of the great beams of the corona; they remain constant, while the latter were variable. There is a diameter approximately corresponding to the solar axis, near the extremities of which the radiance upon the photographs is a minimum, whereas the coronal beams in these directions were especially marked during a great part of the total obscuration. The coronal beams stood in no apparent relation to the protuberances, whereas the aureole seen upon the photographs is most marked in their immediate vicinity. . . . Whatever of this aureole is shown upon the photographs was occulted or displayed by the lunar motion, precisely as the protuberances were. The variations in the form of the corona, on the other hand, did not seem to be dependent in any degree upon the moon's motion. The singular and elegant structural indication in the special aggregations of light on the eastern side may be of high value in guiding to a further knowledge of the chromosphere. They are manifest in all the photographs by your parties which I have seen, but are especially marked in those of shortest exposure, such as the first one at Ottumwa. In some of the later views they may be detected on the other side of the sun, though less distinct; but the very irregular and jagged outline of the chromosphere, as described by Janssen and Lockyer, is exhibited in perfection."

drawing, and make it one of the most valuable of the products of the Expedition.

On former occasions the corona has been stated to assume a roughly four-cornered form. This was also observed in Spain last December, and seems at last explained by three drawings made by one of the American party there.

Shape of the corona.

At the commencement and end of totality, when the moon unequally covered the sun, the photographs have recorded an excess of light on the corona on the side where the limbs occur nearest in contact. I am told that this effect in one of Lord Lindsay's photographs is very striking; it is certainly so in one of Mr. Brothers'. In the drawings we have a slightly different effect. At the commencement of totality, when the western or right-hand limbs were in contact, we get Fig. 107 (1); at the end of totality the appearance recorded was like 2; the picture at the middle of totality compounding both these appearances, and being roughly represented by 3, in which the rectangular appearance comes out in its full strength.

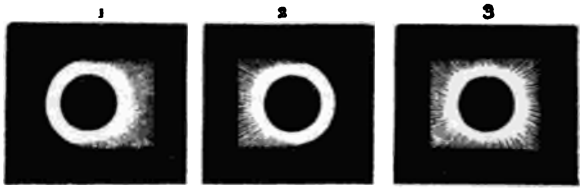

FIG. 107.—Why the corona was observed square.

A word now about the polariscopic observations. I may remark on this that it is much more easy for us to explain slight polarization which might be atmospheric, than it is to explain, if we content ourselves with laboratory experiments, strong *radial* polarization which must take place at the sun. If we assume that gas or vapour of considerable tenuity does not reflect light (although I think this is to assume very much for the gas or vapour *at the sun*, at all events), what is it that reflects light to us at the sun, and reflects it apparently only *above* the level of the intensely incandescent hydrogen? Certainly not solar spray. If we deny reflection to gases altogether, may it not be the continuous portion of the spectrum of the gas itself to which the light is due? But this question of polarization is certainly one in which very much remains to be done, and it is consoling to know that the results obtained now will much facilitate the planning of the next polariscopic campaign, which, we may add, should not be deferred beyond the end of this year.

THE STORY OF THE CORONA IN CONNECTION WITH THE MEDITERRANEAN ECLIPSE.[1]

CHAP. XVIII.

MY present duty, a pleasant one, although it is tinged with a certain sense of disappointment,[2] is to state the observations which were made of the recent eclipse in Spain and Sicily, to connect them with our former knowledge, and to show in what points our knowledge has been extended. In these observations, we had nothing to do with the sun as ordinarily visible, but with the most delicate phenomenon which becomes visible to us during eclipses. I refer to the *corona*.

General Notions of the Corona.

Let me, in the first place, show what is meant by this term, and state the nature of the problems we had before us. I have here some admirable drawings, which I will show by means of the lamp, of the eclipse that was observed in 1851 by several astronomers who left England in that year to make observations in Sweden, where the eclipse was visible. It must be borne in mind that the drawings I shall bring to your notice were made in the same region, at places not more than a few miles apart.[3]

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[1] A Lecture delivered at the Royal Institution of Great Britain, Friday evening, March 17, 1871.

[2] This refers to the fact that the weather was generally bad, and that it was impossible to make observations during the eclipse.

[3] Mr. Carrington observed at Lilla Edet, on the Gota River. The Astronomer Royal observed at Göttenburg. The second drawing

The drawings of the 1851 *corona by Mr. Carrington and the Astronomer Royal.*

The first drawing was made by an observer whose name is a sufficient guarantee for its accuracy—I refer to Mr. Carrington—*and when the sky was absolutely free from clouds.* In the next diagram you will see the corona is changed. The bright region round the sun is no longer limited to the narrow border of light round the dark moon, as seen by Mr. Carrington, but it is considerably expanded.

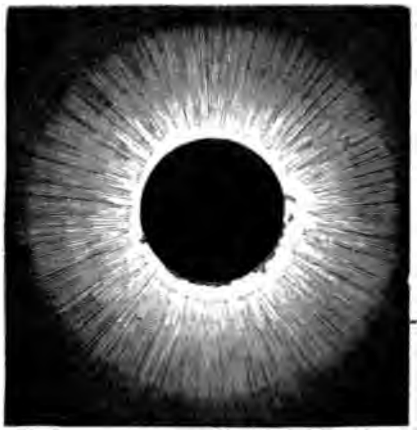

FIG. 108.—Total Eclipse, 1851. (Dawes.

The third gives still a greater extension, although that picture was drawn within a quarter of a mile of the place where Mr. Carrington's was taken. And lastly, we have a drawing made by the present Astronomer Royal, of that same eclipse, *through a cirro-stratus cloud,* as unlike Mr. Carrington's as anything can possibly be. So that you see we began with a thin band of light about the moon, which would make the corona a few thousand miles high, and we end with a figure which Mr. Airy graphically likens to the ornament round a compass-card, and which gives the

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referred to was made by Petterson, at Göttenburg; the third by a friend of the Rev. T. Chevallier, at the same place; and I might have added another by Fearnlay, taken at Rixhöft, in which the corona is larger than in any of the others. The series is most instructive. See *Mem. R. A. S.*, vol. xxi.

corona a height equal to about once and a half the sun's diameter.

I will next bring before you some drawings made during the eclipse of 1858, which was not observed in European regions, but in South America by two first-rate observers—one, M. Liais, a French astronomer, who was stationed at Olmos, in Brazil; the other, Lieutenant Gilliss, who was also there as a representative of the American Government, and observed some thousand miles away in Peru.

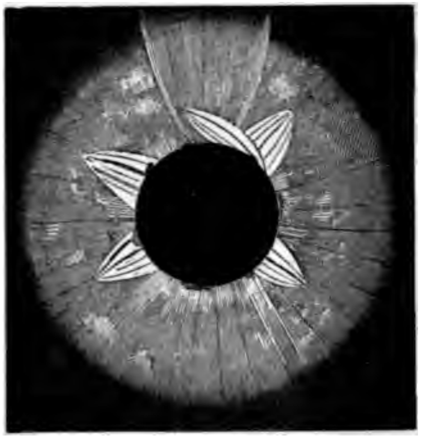

FIG. 109—Total Eclipse, September 1858. (Liais.)

Drawings of the South American eclipse of 1858.

I will throw on the screen the appearances observed by these gentlemen, and I think you will acknowledge the same variations between their results, as to degree, while in one case we get a perfectly new idea of phenomena—a difference in kind. I would especially call attention in the Olmos drawing to those extraordinary bundles of rays of wonderful shapes, which you see are so much brighter than the other portions of the corona. Such forms have been seen in other eclipses, but they are somewhat rare. The drawing made by Lieutenant Gilliss bears the same relation to that made by M. Liais as Mr. Carrington's did to the

Astronomer Royal's; so that we may say that we not only get variations in the dimensions of the corona as seen at different stations, but that we furthermore get a strange structure introduced now and then in our drawing in regions where absolutely no corona at all exists in the other.

So much by way of defining the phenomena and giving an idea of the eye observations generally.

Let me now attempt to show you how the phenomena observed in the last eclipse bear upon the results which had been previously accumulated by means of telescopic and naked-eye observations, and by means of the polariscope and spectroscope.

I. TELESCOPIC AND NAKED-EYE OBSERVATIONS.

a.—A Part of the Corona is undoubtedly Solar.

Portions of the corona have been seen when the sun was uneclipsed.

The first use I propose to make of the telescopic and naked-eye observations of last year, is to show you a photographic copy of an admirable drawing made by Mr. Brett, who, though unfortunate enough to see the sun only for a very short time, was yet sufficiently skilled to make good use of that brief period. This drawing will bring before you the fact, that even when a large portion of the sun remained unobscured by the moon, Mr. Brett was enabled to see a dim ring of light round the unobscured portion, which since the year 1722[1] has been acknowledged, beyond all question, I think I may say, to represent something *at the sun.* It was observed in 1722 round the uneclipsed sun, and in more recent times by Mrs. Airy in 1842, and by Rumker 1½ minute before totality in 1860, not to mention other instances. Therefore, we have one observation made during this eclipse, confirming the old one, that in the corona there is a region of some small breadth at all events which is absolutely solar, and which it only requires a diminution of the solar light to enable us to see. This, then, we may look upon as the known; now let us feel our way gradually outwards.

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[1] For this and the following points the admirable chapter on Eclipses in Grant's "History of Physical Astronomy" should be consulted; as also Madler's review of the observations made in 1860.

b.—Rays, or Streamers, are added at Totality.

The drawings made in all the eclipses which have been carefully recorded bring before us quite outside this narrow, undoubtedly *solar region*, observed before totality, as I have shown, and also by Mr. Carrington and by Lieutenant Gilliss during totality in 1851 and 1858, extraordinary appearances of a different order. While in fact we have a solar ring from 2′ to 6′ high, we have rays of all shapes and sizes visible outside, in some cases extending as far as 4°, and in all cases brighter than the outer corona on which they are seen, the rays being different in different eclipses, and appearing differently to different observers of the same eclipse, and even at the same station. Here is a copy of a drawing made by M. Rumker of the eclipse of 1860,

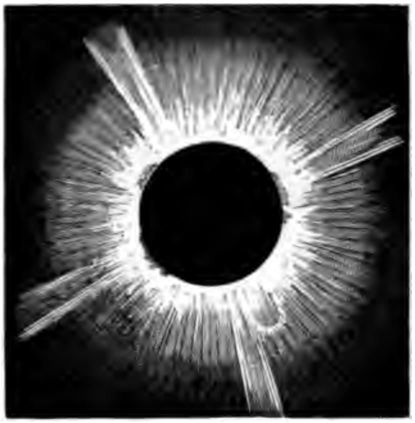

FIG. 110.—Total Eclipse, 1860. (Rays observed by Feilezsch.)

Different sides of the corona ar. more developed at different times during an eclipse.

and I show it for the purpose of calling your attention to the fact that the two curious rays represented in it belong to a different order of things from those which we see in the rest of the corona. From the beginning to the middle of the eclipse the east rays were the most intense. In the next drawing, which was made by the same observer, you see something absolutely new: and now the western side of the corona is the most developed; we have a new series of bright rays, and altogether it is difficult to believe that it is a drawing made by the same observer of the same eclipse.

The third drawing is a representation of the same eclipse by M. Marquez, who observed with a perfection of minute care which has scarcely ever been equalled; I bring it before you to show that the rays he saw were altogether differently situated. We may conclude, then, that the rays, although extremely definite and bright—as bright or brighter than the other portions of the corona which are visible before totality, they being *invisible* before totality—appear different to different observers of the same eclipse, and to the same observer during different *phases*.

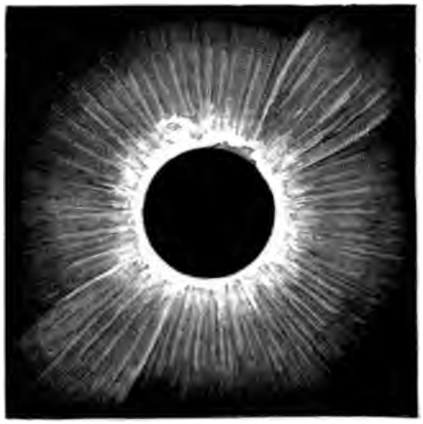

FIG. 111.—Rays observed in the Total Eclipse, 1840.

c.—They change from Side to Side.

I have already said that M. Rumker observed that from the beginning to the middle of totality the rays on the *east* side of the sun were longest and brightest, and that from the middle to the end of totality the rays on that side of the sun where the totality ended were longest and brightest.

Changes during progress of an eclipse.

We will now carry this observation a step further, by referring to three drawings made by M. Plantamour in the same eclipse, that of 1860. In the first drawing we have the beginning of the total eclipse as seen in the telescope: with the naked eye naturally we should get the sun disappearing at the east or left-hand side, the moon moving from west to east; in the telescope things are reversed, and we have it right instead of left: and here we have the same thing that M. Rumker observed, namely, that when the eastern limbs were in contact, bright rays (M. Plantamour saw three) were visible on the side at which the contact took place. When the moon was half-way over the sun, *two* rays of reduced brilliancy were observed on that side, not necessarily in the same position as *those* first observed, but one of *these* has been abolished altogether; and on the other side of the sun, where totality was about to end, we have three rays gradually suggesting themselves: at the end of totality the rays visible at the commencement are abolished, and now, instead of them, and of those seen at the middle of the eclipse, we have a bran new set of rays on the side of the moon from whence the sun is about to emerge.

This observation, I need hardly say, is of considerable importance in connection with the fact that from the year 1722 almost every observer of a total eclipse has stated that there is a large increase of brilliancy, and an increase of the size of the corona on the side where the sun has just been covered, or is just about to emerge.

Now, what was there bearing on this point in the recent observations? I have here three drawings, which, though roughly done, you will see are of great importance side by side with those of M. Plantamour. These are drawings which have been sent in to the Organizing Committee by Mr. Gilman, who lives in Spain, and who took considerable interest in the eclipse, and sent the results of his observations to England with the eclipse party when they came home; and it is of importance that you should see everything that Mr. Gilman has done. If you agree with this explanation of the square form of the corona, which was observed in Spain this year, it will explain the quadrangular form observed in the corona in a good many previous eclipses. Mr. Gilman says that at the commencement of totality—let me remind you, the commencement was determined by the disappearance of the sun at the east limb of the moon, which is east in Mr. Gilman's drawing, as he was observing with the naked eye—the commencement, he says, was determined by the corona flashing out very much like a capital D. You see on the black-board exactly the outline, and you will at once mentally associate one-half of the diagram with the rays observed by M. Plantamour, and the other half, in which there is a nearly perfect ring of light round the moon, with the corona observed by Mr. Carrington all round it in a cloudless sky. At mid-eclipse, Mr. Gilman also observed the corona, sketched out its outline carefully, and found rays coming out on the opposite side, adding themselves on to the perfect ring first seen there. Opposite the two salient angles he observed at the commencement of totality—represented by the top and bottom of the upright stroke of the capital D

Mr. Gilman's drawings.

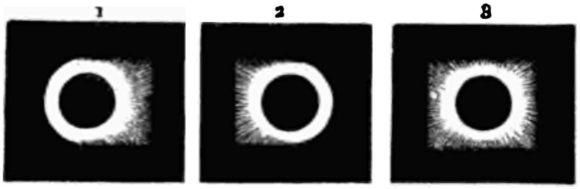

FIG. 112.

—there were two others; *the corona now appeared square,* and then, just before the end of totality came on, the two corners first seen were observed to disappear altogether, leaving nothing but a perfect ring, and where, at the beginning of the eclipse, nothing was seen but a perfectly round ring, the two exactly similar forms on the opposite side shot forth, and you got a D reversed (ᗡ). Mr. Warrington Smyth, who drew a square corona, saw the light flash out into the corona before the end of totality, and believes that all the angles of the square were not visible at one and the same time.

Here, then, you have observations of exactly the same character as those of M. Plantamour, to which I have referred. In the drawings of both are shown the inner part of the corona, which you saw growing in the observations of 1851, to which were added the strange forms observed in 1858. You have these strange variations positively growing at the same place, and the same time, in the same and in different eyes. Obviously there must be very much that is non-solar—call it personality, atmospheric effect, or what you will—connected with it. We have added to the stable the unstable. The question is, to what is this unstable portion due?

d.—They are very variously represented.

I will now refer to other drawings of the late eclipse, which were made in Sicily. For some reason or other, which I do not profess to understand, the corona, which appeared in Spain to be square, and to Mr. Gilman like a D at the beginning, and like a D reversed (ᗡ) at the end,—to all those with whom I have conversed who saw it in Sicily, it appeared as round as you see it here, in this drawing made by Mr. Griffiths; and, instead of being square, we had sent to us all sorts of pictures, a large number of them representing a stellate figure. Here is a drawing made by a Fellow of the Royal Society, on board one of Her Majesty's ships (the *Lord Warden*) which were trying to save the poor *Psyche* at Catania. In this we have perfectly regular rays drawn from every region of the sun, some long, some short, but similar rays are almost invariably opposite each other; but in the interior, inside these rays, the corona is just as it was observed by Mr. Griffiths at Syracuse. I now show you a drawing made by an American gentleman at sea, between Catania and Syracuse, with one ridiculously long ray, a ray as long as was seen by Otto Struve in 1860. Other drawings were made, even on board the same ship, so unlike each other, and so *bizarre*, that I need only refer to them as showing that there at all events must be some personality. We have, then, to account for the variations between the observations made in Spain and those made in Sicily. I regret that we have not a third order of difficulties to contend with, as doubtless we should have had if observations had been made by Mr. Huggins' party in North Africa.

An excessively long ray observed.

e.—The Rays are accompanied by a Mass of Light.

These changes of the rays from side to side are accompanied by, and are perhaps to a certain extent due to, the bursting forth of brilliant light in their neighbourhood, where the limbs are nearest in contact. This was first observed by Miraldi in the eclipse of 1724, and has frequently been recorded since. Mr. Warrington Smyth, to whom I have before alluded, states that he noticed this in the last eclipse, and the photographs, I think, have recorded it; but as there is some uncertainty on this point, I need only suggest it.

f.—Long Rays are seen extending from the Cusps before and after Totality.

So far I have referred only to the rays visible during totality, but long rays were seen, when a crescent of the

Rays from the cusps not solar.

sun was visible in 1860 and 1868, by Mr. Galton and Mr. Hennessy. Mr. Brett caught the same phenomenon last year; but as the sky was cloudy, the commencements of the rays only were seen, appearing like delicate brushes in prolongation of the cusps. These observations are of great value, *as no one for one moment imagines that these rays are solar,*[1] and yet they are very like those seen during totality.

g.—Sometimes Dark Rays, called Rifts, are seen instead of Bright ones.

These rays to which I have referred are, however, not the only kind of rays that are observed. At times are seen, as it were, openings in the corona; the openings being of the same shape as the rays, that is, expanding as they leave the dark moon, and opening more or less exactly as the rays do. Like the rays also, they are sometimes very numerous; in other eclipses they are few in number. Let us take the eclipse observed in India in 1868. Several drawings made there showed the corona as square as it was drawn in Spain last year; others as round as it was seen in Sicily; but the eclipse was not observed only in India, it was observed at Mantawalok-Kelee by Captain Bullock, and at Whae-Whan, on the east coast of the Malayan Peninsula, by Sir Harry St. George Ord, Governor of the Straits Settlements.[2] In the former place we had rifts expanding rapidly as they left the sun—one forms an angle of 90°, the sides of another being *parallel*—separating patches of corona, which in some places extends 2½ diameters of the moon from the sun.

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[1] The probable origin of these rays will be stated in the sequel.

[2] For the observations made by this party see the "Account of a Visit to the King of Siam," printed at the Government press at Singapore, in 1868. The rifts are thus described: "It was noticed that from several points in the moon's circumference darker rays emanated, extending to a considerable distance into space, and appearing like shadows cast forth into space by something not very well defined."

The dark rays or "rifts."

At Whae-Whan we are told that at one particular moment of the eclipse "it was noticed that from several points in the moon's circumference darker rays emanated, extending to a considerable distance into space, and appearing like shadows cast forth into space by something not very well defined;" these dark rays afterwards "diminishing."

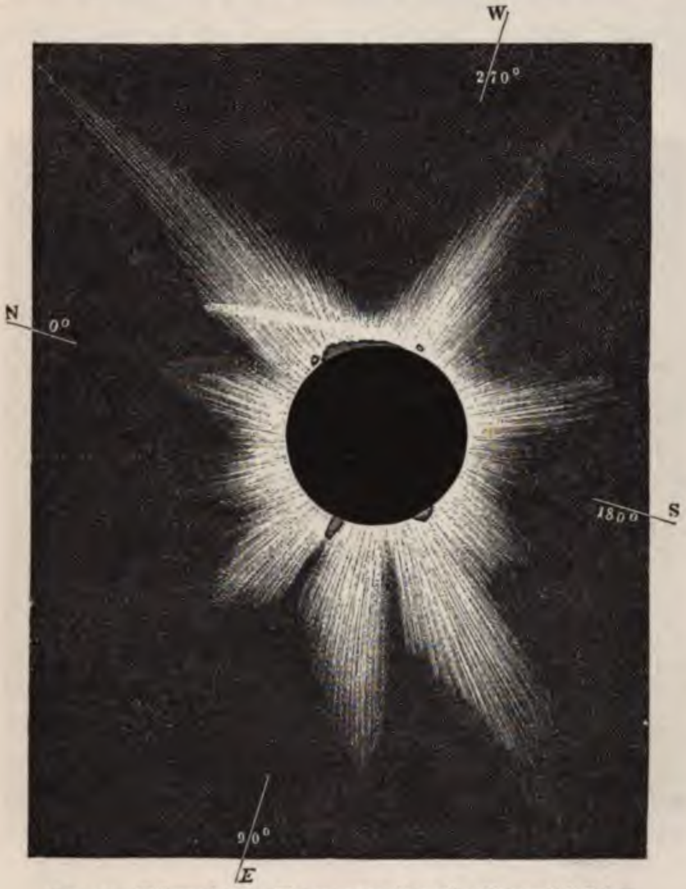

FIG. 113.—Eclipse of 1868. The corona as observed at Mantawalok-Kekee.

Variation in colour of the rays.

Now let us pass on to the eclipse of 1869. In two drawings made by Dr. Gould, in which the changes in the bright bundles of rays come out in a most unmistakable way, we get similar rifts, which changed as violently as did the rays; while in another drawing made by Mr. Gilman the whole corona is furrowed by narrow rifts in all regions lying between violet, mauve-coloured, white, and yellowish white rays!

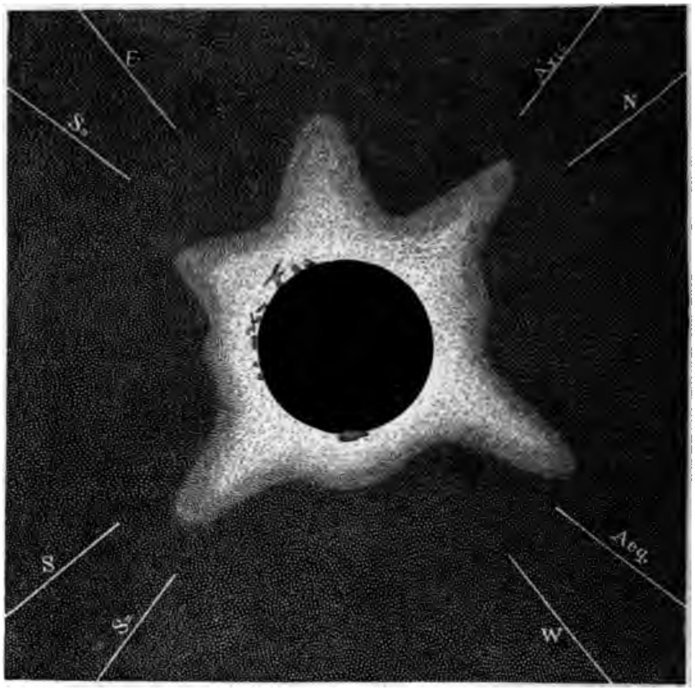

FIG. 114.—Gould's drawing of the corona, near the beginning of totality, at Burlington.

Now, what have we bearing on this point in the recent observations? No rift was *seen* in Sicily; one rift was recorded by the sketchers in Spain, but more than one rift was photographed in both places. We must remember, however, in thus bringing eye-sketches and photographs into comparison, first that the eye too often in such observations retains a general impression of the whole phenomenon, while the plate records the phenomenon as it existed at the time at which it was exposed; and secondly, that we know that the plates record chemically, while the eye records visually. We are dealing with two different kinds of light.

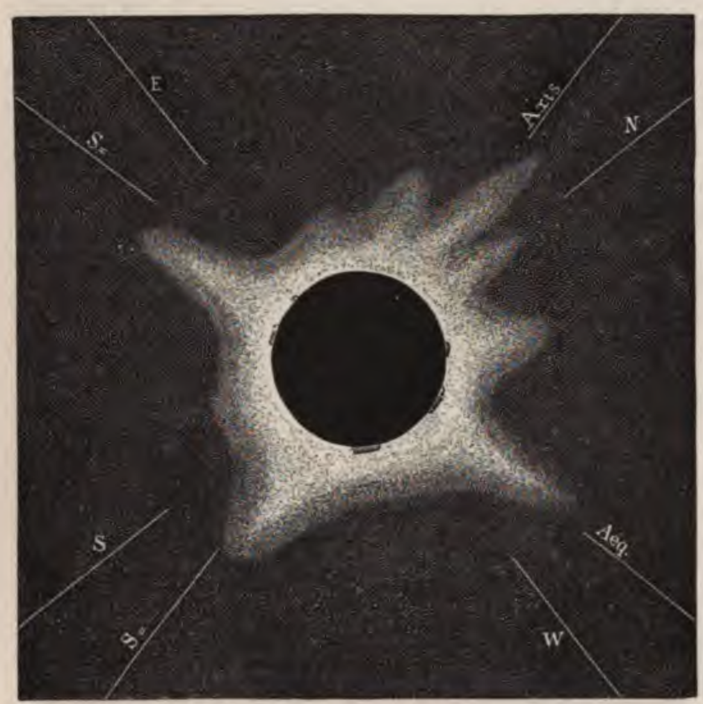

FIG. 115.—Gould's drawing of the corona, near the end of totality, at Burlington.

The photographs.

I will show you two photographs on the screen. Although the lucid intervals were very rare, we were fortunate enough to get one photograph of the coronal regions in Syracuse, and one in Spain. I now show you the photograph made

One Spanish photograph shows rifts,

by the American party in Spain. You see here that, probably owing to a cloud, we get a certain amount of light driven on to the dark moon, and you also see the indications of the rifts. This photograph was taken with an instrument with a small field of view, so that the most important parts of the corona were rendered invisible by the instrument itself.

the other does not.

Lord Lindsay, who also photographed in Spain, recorded no rifts.

In the other photograph, taken at Syracuse, the result is better. We have the equivalent of the rift in the photograph I showed you before. The instrument was extremely unsteady, and the definition not so good as it would have been if Mr. Brothers had had a good opportunity of dis-

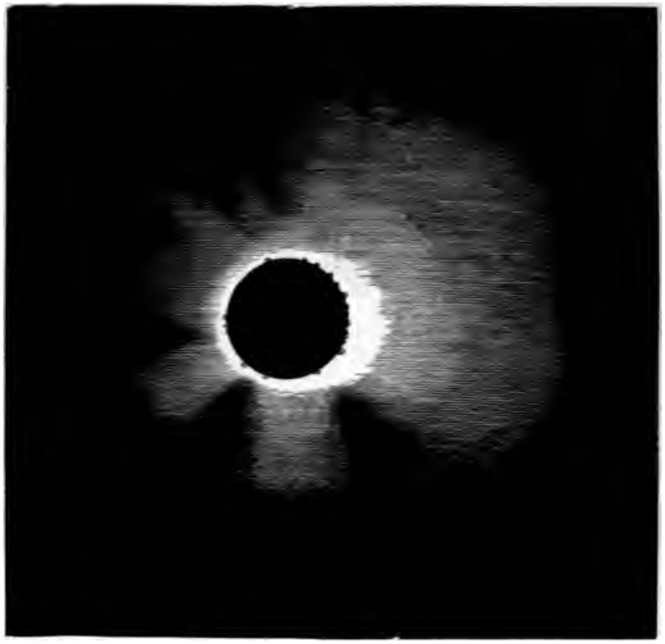

FIG. 116.—Eclipse of 1870. Photograph of the corona taken at Syracuse.

playing his skill. We get other fainter indications of other rifts here and there, and the question whether these rifts agree in the photograph taken in Spain with those in that taken in Syracuse is one of great importance; and it is to be hoped that before long it will be set at rest. Some observers think they agree; others think they do not.

But there is an important consideration based on that photograph, to which I must draw your particular attention. I have shown you the photograph as it may be thrown on the screen; but in the photograph itself there are delicate details which it is impossible to reproduce. The dark portions in the corona indicated in the copy I have shown you are merely the bases of so many dark wedges driving out into space, like their prototypes in the Indian eclipse. It is Mr. Brothers' opinion, I believe, that all you see on the screen round the dark moon, all that enormous mass of light, nearly uniform in texture, and these beautiful broad rays between the rifts, are really and absolutely parts of the solar corona. I confess I do not wish to commit myself to such an opinion. We want more facts, and the *onus probandi* lies with those who insist upon that view, and I have yet to hear an explanation of them on that basis.

Opinion reserved.

h.—The Corona sometimes seems to be flickering or rotating.

We now come to the next point. Time out of mind—that is, for the last two centuries—the corona has been observed to be flickering, wavering, or rotating, moving in every conceivable way and direction. In 1652 it was described as "a pleasant spectacle of rotatory motion."[1] Don Antonio Ulloa remarked of the corona observed in the

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[1] For many of these references I am indebted to Grant's "History of Physical Astronomy," which should be consulted by the reader.

Corona observed to rotate in 1652 and in 1788.

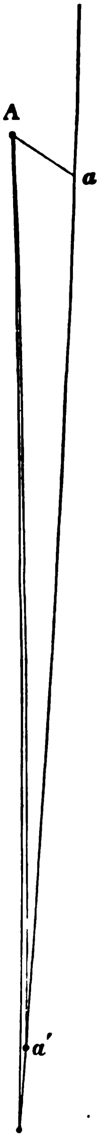

Fig. 117.

eclipse of 1788, "It seemed to be endued with a rapid rotatory motion, which caused it to resemble a firework turning round its centre." The terms whirling and flickering were applied in the eclipse of 1860. This extraordinary condition of things was also thoroughly endorsed by the late observations. It certainly exists, and is among the observations we have to take into account. When I saw an officer of one of the ships at Catania, I asked him if he had taken a drawing of the corona. "No," he said. I asked him, "Did you see any rays?" "Yes." "Then why did you not make any drawing of them?" His answer was, "How on earth could you draw a thing that was going round and round like a firework?" This was not the only observation of the kind, and the tendency of such observations, I need hardly say, is to strengthen a belief in the unstable, and therefore uncosmical, nature of these rays.

Is this variation of light due to the brilliancy of the corona, and the rapid change of the rays, which is one of the results which comes out clearest? In 1842 the brilliancy of the corona was stated to be insupportable to the naked eye. A similar remark was made to me by several of those officers who saw the last eclipse in Sicily.

II.—POLARISCOPIC OBSERVATIONS.

With regard to the polarization experiments, by the kindness of Mr. Spottiswoode I am enabled to show you, in a very clear way, the *raison d'être* of the polariscopic observations made during this and former eclipses; but the polariscopic ground is a wide one, and it is not my intention to cover it to-night.

I have had this arrangement of lamp, reflector, and prisms made, so that you may see how the polariscope can determine the percentage of reflected light at different angles, and the direction of reflection. Assume this lamp (A, Fig. 117) to represent the sun; let this reflector *a* close to the lamp represent a particle near the sun, reflecting light to the eye B: we shall naturally have the light reflected at a much larger angle than if the reflector *a′* representing a particle in our own air were close to the screen. Having this idea of the angle of reflection in your minds, and the fact that the larger the angle under these conditions the more the polarization, if you take this lamp, as I have said, to represent the sun, and this mirror to represent any particle, of whatever kind you choose to imagine, it is clear that in order to get the maximum polariscopic effect from that particle, you must have it so situated that it will reflect light at a considerable angle to the beam coming from the electric lamp.

The angle of reflection is large when the reflecting particle is near the source of light.

Now it is clear that, in order to polarize the beam most strongly, I must place the reflector close to our imaginary sun. If I so place it as to represent a particle in our own atmosphere, the angle will be so small that the polarization of the light will hardly be perceptible.

Here is our sunlight, which we will polarize at as great an angle as we can, by placing the reflector close to the imaginary sun, and send it through this magnificent prism which Mr. Spottiswoode has been good enough to place at our disposal; and in the path of the beam I will place an object, so that you may determine whether there is polarized light. [Experiment.] You see there is considerable brilliancy in those colours; their brilliancy depending upon the amount of polarization.

Now if, instead of having our reflector close to our imaginary sun representing a particle in the sun's atmosphere, we place it near the screen to represent a particle

Small when away.

in our own, in which case the angle is extremely small, the brilliancy of the colours will entirely disappear. You see it has disappeared. The colours, as colours, are distinguishable, but their brilliancy has gone.

That is the *rationale* of the polariscopic observations which have been made on the occasion of the last eclipse, with more elaboration than they ever were before. If we found the corona to be strongly polarized, this was held to be a great argument in favour of the corona being a real solar appendage, an argument strengthened if the polarization was also found to be radial. At present, however, a great many of the observations that have been made have not been received, and those that have been received are as discordant as those obtained in former eclipses, and therefore my account is an imperfect one, because I have not had an opportunity of discussing all these observations. Indeed, if I had, I should hesitate to give an opinion on the subject. When Mr. Carrington saw that small corona in 1851, and Mr. Gilliss saw that small corona in 1858, neither of them traced any polarization whatever; but when M. Liais saw that large corona in 1868 which was invisible to Mr. Gilliss, he in his turn saw an immense amount of polarization, which led him to believe that the corona was solar, the whole of it, rays and everything included, and that we had an indication of a solar atmosphere two or three times higher than the diameter of the sun; that is, an atmosphere two or three millions of miles in height.

The polariscopic results are doubtful.

This observation is not in accordance with the general conclusions from the drawings I have shown you; and let me add that the assumption of reflection at the sun is not without its difficulties, and that we have not yet traced reflected sunlight, even when the strongest polariscopic effects have been observed.[1]

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[1] See the chapters on the Indian Eclipse of 1871. It is there shown that M. Janssen has obtained evidence of the reflected sunlight. (1873.)

III.—AIRY'S AND MÄDLER'S CONCLUSIONS AS TO THE RESULTS OF THE PRE-SPECTROSCOPIC OBSERVATIONS.

Before passing to the spectroscopic observations I will state the conclusions at which the Astronomer Royal and M. Mädler arrived after the observations of 1860 had been gathered together.

The Astronomer Royal, in a lecture delivered before the British Association at Manchester in 1861, stated that the assumption of an atmosphere extending to the moon explained the observation of Plantamour, which could, he thought, be explained in no other way, and he held also that the polarization experiments seemed to show the same thing. The Astronomer Royal was content to find the reflection, which so many now insist must be at the sun, taking place somewhere between the earth and moon.

M. Mädler's verdict is in the same direction, and though he does not perhaps express so decided an opinion, he maintains that the atmosphere plays a principal part in the phenomenon; and after detailing experiments to show this, he remarks of the solar and atmospheric portions, "Both cover each other and unite in one phenomenon, so that the corona is a mixed phenomenon."

The corona is a mixed phenomenon.

I shall shortly show you that the spectroscope, leaving the telescope out of consideration, has taught us that this is true, though I shall not be able to show you that it is the whole truth; we are not yet in a position to do that. Mädler[1] concludes his observations by remarking, "We cannot share the doubts of those who are afraid to surround the sun with too many envelopes; neither do we find anything unnatural in the statement that the sun has as many atmospheres as Saturn has rings; but we gladly admit that we cannot yet say anything positive. We have here a large field of probabilities, and the decision may yet be distant." We can speak with more certainty now!

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[1] See Mädler's papers in vols. xxviii. and xxix. of the *Transactions of the Jena Academy.*

IV.—SPECTROSCOPIC OBSERVATIONS.

a.—Spectrum of the Corona first observed by Tennant, Pogson, and Rayet.

We now come to the consideration of those observations in which we are aided by a most powerful and our most recent ally, the spectroscope, first used in the eclipsed sun, as you know, in the eclipse of 1868. You all know that in that year the question of the nature of red flames was for ever settled by M. Janssen, Major Tennant, Captain Herschel, and others, who observed that eclipse in the most admirable manner; but we have nothing to do with the red flames now, we have to do with something outside them.

The spectrum of the corona said to be continuous.

Now, most of you are under the impression, and it was mine until the day before yesterday, that the only thing we learnt about the corona in the eclipse of 1868, was that its spectrum was a continuous one; and I need not tell anyone in this theatre that the assertion that it was continuous was one that was extremely embarrassing, and implied that we had something non-gaseous outside the red flames, which seemed very improbable to those who know anything about the subject. But some of you will no doubt remember that, besides Major Tennant, who made this observation, we had a French observer, M. Rayet, who gave us a diagram of the spectrum of one of the prominences, and Mr. Pogson, who has now been for some time in India, and is a well-known observer, who gave us, nominally as the spectrum of a prominence, a spectrum with some curious variations from M. Rayet's diagram.

I exhibit a copy of M. Rayet's diagram[1] of the spectrum of a prominence, as he called it. At the bottom is what

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[1] The following is M. Rayet's account of his observations :—

"La fente du spectroscope ayant été replacée perpendiculairement au bord du soleil, c'est-à-dire, dans la direction de l'axe de la protubérance en forme de corne, je retrouvai les neuf lignes brillantes vues un instant auparavant. Toutes ces lignes prenaient naissance au même niveau vers la partie du champ spectral occupé par l'image de la

he considered to be the spectrum of the lower portion of the prominence, while in the higher portion, where we get fewer lines, as he considered, is the spectrum of the higher portion of the prominence; the spectrum of the lower portion contains the lines B, D, E, and F, and some other lines, in all nine, while the spectrum of the upper part of the prominence, as he thought it, only contains three lines. It was at first difficult to account for these observations. In the first place, one could not understand the line B being given, because I soon found that the line B was not seen as a bright line in the chromosphere spectrum; it was clearly the line C that was intended. Hence

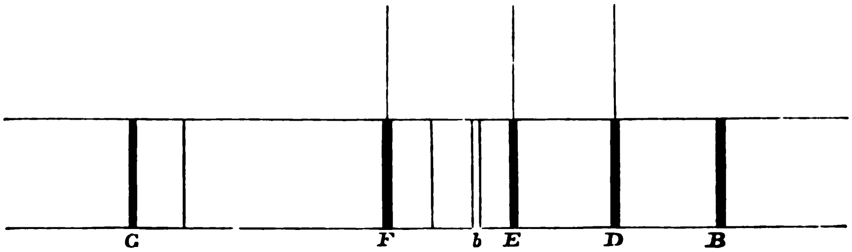

FIG. 118.—Copy of Rayet's diagram.

lune ; mais du côté qui répondait à la partie la plus élevée de l'atmosphère solaire, ou au sommet de la protubérance, elles se terminaient à des hauteurs variables ; quelques-unes d'entre elles se prolongeaient au-delà de la longueur moyenne par un trait lumineux très-faible. Ces diverses lignes différaient d'ailleurs par leur éclat relatif. Le fac-simile ci-joint (Fig. 118) d'un dessin fait peu d'instants après l'éclipse lorsque mes souvenirs étaient très précis, reproduit l'apperance générale du spectre.

" D'après la disposition des lignes dans le champ, d'après leur espacement relatif, leur couleur, et enfin, d'après la physionomie même de leur ensemble, j'avais assimilé les neuf lignes brillantes de mon spectre aux principales raies de Fraunhofer, B,* D, E, b, une ligne inconnue, F et deux lignes du groupe G. Cette estime était imparfaite sans doute ; on verra cependant qu'elle n'était pas fort éloignée de la vérité."—*Mémoire sur les Raies brillantes du Spectre de l'Atmosphère solaire et sur la Constitution physique du Soleil.*

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* "Il y a eu ici une erreur d'estime ; la ligne B ne fait pas partie du spectre des protubérances, mais on y rencontre la ligne C."

Observations of Rayet

doubt was thrown on the other lines; it seemed as if M. Rayet was wrong about his elongated lines D, E, and F, and probably meant C near D and F. And so it was explained —I am ashamed to say by myself—that there was no particular meaning in these elongated lines, except that the spectrum of the prominence some distance away from the sun was simpler than it was nearer the sun, as happens in all prominences, as we may now determine any day we choose to look at the sun by means of the spectroscope.

and Pogson.

Now let us hear Mr. Pogson.[1] He gave a diagram showing

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[1] I here give his statement on this point *in extenso.*

"A little before 9 A.M., when the partial phase was well advanced, I turned the spectroscope upon the sun, and took the micrometer readings of seven known lines of the ordinary spectrum, agreeably to Mr. Huggins' instructions. I had previously taken care to have the equatorial stand in tolerable adjustment, and side by side with the Smythian telescope employed for the other class of observations. After measuring the long prominence, as before described, about one minute after the beginning of totality, I directed the finder of the spectroscope telescope to a part of the corona on the sun's southern limb, as clear of any visible prominences as possible. A faint light was seen, scarcely coloured, and certainly free from either dark or bright lines. While wondering at the dreary blank before me and feeling intensely disappointed, some bright lines came gradually into view, reached a pretty considerable maximum brilliancy, and again faded away. Five of these lines were visible, but two decidedly superior to the rest. A turn of the right ascension tangent rod immediately brought back the welcome lines, and by manipulating it with one hand, and the spectroscope micrometer with the other, the readings of the two brightest were secured. It struck me as strange that these brightest lines should appear at a part of the spectrum not corresponding to any very conspicuous dark lines in the solar spectrum ; but not having Kirchhoff's chart in my possession, I must leave it for my scientific friends at home to decide upon the interpretation of the measures obtained. The third line seen, in order of brilliancy, must have been either coincident with or very near the place of the sodium line D, but it was much fainter than the two measured ; while the fourth and fifth lines were extremely faint, and about as close as E and f, but I estimated them to be somewhere near the position of Fraunhofer's F in the solar spectrum. The fact of bright lines being seen at all, shows that the red prominence which produced them was composed of incandescent gas ; but whether similar to any of our known terrestrial elements or otherwise, it would be premature for me to offer any opinion. The red prominence under observation in my spectroscope was not the long one which most other observers naturally singled out for examination, but one of the two seen side by side about the S.E. by E. point of the moon's black disc. I remeasured the seven ordinary lines of the solar spectrum about the five lines in the spectrum of what he thought a prominence, and he writes:—"A faint light was seen [in the spectroscope], scarcely coloured, and certainly free from either dark or bright lines. While wondering at the dreary blank before me, and feeling intensely disappointed, some bright lines came gradually into view, reached a pretty considerable maximum brilliancy, and again faded away. Five of these lines were visible, but two decidedly superior to the rest. . . . . . The readings of the two brightest were secured. It struck me as strange that these brightest lines should appear at a part of the spectrum not corresponding to any very conspicuous dark lines in the solar spectrum. . . . . . [These lines are a little less refrangible than E.] The third line seen, in order of brilliancy, must have been either coincident with or very near the place of the sodium line D, but it was much fainter than the two measured; while the fourth and fifth

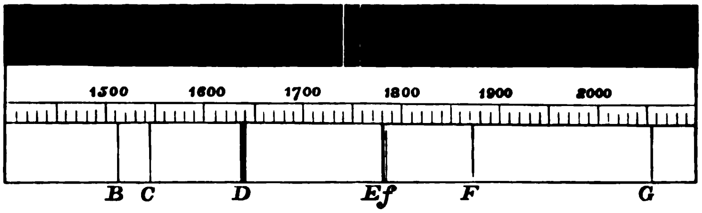

FIG. 119.—Copy of Pogson's diagram.

same time on the following morning, and also repeatedly since my return to Madras, the extreme difference in any case being four divisions of the micrometric scale for the lines A and C. The scale numbers (a mean of the two Masulipatam readings) were as follows :—

a 1513 in the deep red.
C 1547 in the bright red.
D 1639 in the yellow.
E 1782 } both in the green.
f 1785 }
F 1873 in the blue.
G 2055 in the deep blue.

The two bright lines were situated, the most conspicuous at 1743 and the other at 1763."—*Report of the Government Astronomer, Madras, Eclipse of August 18th*, 1868.

lines were extremely faint." [They were very faint and DOUBLED, and near F. I have seen F give way to a double line in our hydrogen experiments, though I am not prepared to say this is an explanation of Mr. Pogson's observations.]

First observations of corona spectrum.

The fact that we have here the first observations of the spectrum of the sun's corona is one beyond all doubt; and why M. Rayet and Mr. Pogson thought they were observing prominences when they were observing above them, is explained by a remark made by Captain Tupman, of the Royal Marine Artillery, who acted as jackal to Professor Harkness, and picked out the brighter spots of the corona for his observation. Professor Harkness, observing the prominence bright lines, said to Captain Tupman, "You have turned the telescope on to a prominence; I want the corona." "No," said Captain Tupman, "I am giving you the corona as well as I can." It was certainly the corona in both cases. Here you see, dimly and darkly, the first outcome of the spectroscope on the nature of the corona; a record as fairly written as anything at the sun can write it: and I am more anxious to lay stress on these observations, since they have lain fallow for two years, and show the importance of observations, not only in extending our knowledge, but in explaining prior observations; and it is an additional reason for never rejecting an observation. What was, however, dim and dark in 1868 shone out brightly in 1869, thanks to the skill of the American observers of the eclipse of that year.

b.—Laboratory Experiments bearing on these Observations.

But before I proceed to refer to the admirable observations made in America during this eclipse, I wish to introduce you to some work which was commenced in 1868, and has been done quite independently of eclipses. In a lecture which I delivered here about two years ago, I described to you some of the facts observed by the spec-

The dispersion necessary to destroy the atmospheric light weakens and shortens the chromospheric lines.

troscope in the bright-line region which had been spectroscopically determined to exist all round the sun, and which, as in it all the various coloured effects are seen in total eclipses, I had named the Chromosphere. It was clear that by the new method of observing this without any eclipse, by partially killing, so to speak, the atmospheric light, we got a percentage only of the phenomenon, as the atmospheric light could only be killed by an amount of dispersion which enfeebled and shortened the chromospheric lines; so that, although we could say that an envelope of some 5,000 or 6,000 miles in height existed round the sun, we could not fix this as a maximum limit. Further, when we examined the spectrum of this envelope we got long lines and short lines; and I told how the short lines indicated a low stratum, and how a long line indicated a higher one. To explain this, I will show you an observation made long before the new method was thought of. Even before that time we had abundant evidence of such strata, if we could not determine their nature: we had distinct evidence either of one thing *thinning* out, and then another, or that various substances were situated at different levels, under different conditions; on the first hypothesis, at the extreme outside of the chromosphere the last thing would thin out, and then there would be an end of all things as respects the sun.

Schmidt's drawing.

I will show you a drawing made by Professor Schmidt of the eclipse of 1851. I do not wish to call your attention to the strange shape of the large prominence, but to the fact that as the moon passed over this region we get a thin red band, first along the edge of the dark moon, and after the moon had passed over still further we see this red layer, *suspended as it were in the chromosphere*, with a white layer below it. This is the explanation of the long and short lines visible in the spectrum of the chromosphere: in the red layer we have hydrogen almost alone; below, its red light was conquered by other light with bright lines in all parts of the spectrum, and we get white light.

Lord Lindsay tells me he has a distinct indication, written by the sun himself, that in one particular part of the chromosphere, as recorded photographically in Spain, there were three such layers. And over and over again we find recorded white light close to the sun, then red alone, or red mixed with yellow, then violet, and lastly green. And M. Mädler remarks on this very admirably, "The violet band is the link between the prominences and the corona."

There must be cool hydrogen outside the chromosphere.

Before going further, I will show you the difference in the appearance of what we may term hot hydrogen and cold hydrogen; that is, hydrogen which we drive into different degrees of incandescence by means of the spark. After Dr. Frankland and myself were able to determine that the pressure in these solar regions was small, we came to the conclusion, that outside the hot hydrogen there must be some cooler hydrogen, in order that the phenomena we observed, both in the laboratory and in the observatory, should agree.

I have in this tube hydrogen at a certain pressure, and here we have a coil which will enable us to send a spark through it; you see we get a certain amount of redness in that tube, and if you look on one side or above you will see a sort of bluish-green light. Now that redness represents the condition of the hydrogen in the region of the sun where Dr. Schmidt gave us that extremely thin red ring, and the combination of the blue and red would give you something very like violet.

Low temperature hydrogen.

But here I have hydrogen under a different condition. In the tube its rareness is not excessive; but in this globe, of which I am about to speak, you have the nearest approach to a vacuum ever obtained through which a spark will pass; and I beg to call your attention to what will now happen. This globe contains the same chemical element, prepared at the same time as the chemical element you have in the tube; but you see that, so far as colour goes, we have something perfectly different in this case. Now we send the spark through it. I would beg Professor

Tyndall, if he will be good enough, to observe the spectrum of this hydrogen in this globe. [Professor Tyndall did so.] You will see that there is one line? [*Professor Tyndall.* Yes.] And a continuous spectrum? [*Professor Tyndall.* And a continuous spectrum.] Cool hydrogen gives us only the bright line F, plus a continuous spectrum, and many of you will know the extreme importance of that observation. It accounts for the F line being observed without the C line in 1868 and last year, and also for the continuous spectrum observed in the Indian eclipse.

c.—The American Eclipse.

When we come from the Indian to the American eclipse with the considerations to which I have drawn your attention—namely, the existence of these different layers due to the different elements and conditions of the same element thinning out—we shall see the extreme importance of the American observations, for they establish the fact that outside the hydrogen layer there was a layer giving only a line in the green, the line which Rayet and Pogson had observed associated with the hydrogen spectrum and the spectrum of the yellow substance. Here obviously we have, I think, merely an indication of another substance thinning out, in spite of the extraordinary suggestion which was put forward that the corona was nothing but a ***permanent solar aurora.***

The outer layer of the chromosphere consists of an element lighter than hydrogen.

I need hardly tell you that the idea of a permanent aurora anywhere was startling, and that of a permanent solar aurora more startling still; but what I claim is that during last year's observations we made this very startling idea into a most beautiful fact—namely, that the outer layer of the chromosphere is in all probability nothing more nor less than an indication of an element lighter than hydrogen, although this is not yet absolutely established, for the line is coincident with one of the lines in the spectrum of iron.

d.—The Layers increase very rapidly in Density. Reproduction of the coloured Phenomena.

Dr. Frankland and myself were early drawn to consider the solar nature of the large coronas, to which I have called your attention, as extremely questionable, even on the supposition of cool hydrogen, because we did not see how, with its temperature and pressure, it could extend very far: and an experiment which I have to make here will probably make that clearer.

We have in these glass vessels hydrogen, a little more brilliant now the spark passes through it than that you saw in the globe, because I have been compelled to mix with it a certain amount of mercury vapour. Below, we have at the present moment sodium vapour being generated from metallic sodium in one tube, and mercury vapour in the other. I hope, if the experiment succeeds, you will see that a good many of the coloured phenomena seen in the chromosphere during eclipses may be easily reproduced by such experiments as this; and not only the coloured phenomena but the *increase of brilliancy* accompanied by changes of colour recorded. You can now all see the yellow tinge at the bottom of one tube, and the green tinge at the bottom of the other; and if there were time to continue this experiment by increasing the density of the vapours now associated with the hydrogen, I could make the bottom portion of each tube where the vapours are densest shine out almost like the sun, while the cool hydrogen at the top would remain not more brilliant than it is at present. We should have, as it were, a section of the chromosphere.

V.—CONCLUSION.

I will proceed now, if you will allow me, to some of the general results obtained during the last eclipse.

I think that, although the work has been very unfortunately interrupted, the result has been most satisfactory. By putting together observations here and observations there, I consider our knowledge of the sun is enormously greater than it was a few months ago. For instance, we are enabled to understand the long-neglected observation of Rayet, and the equally long-neglected observation of Pogson; and we know that outside the hydrogen there is, in all probability, a new element existing in a state of almost infinite tenuity. And we are sure of the existence of cool hydrogen above the hot hydrogen, a fact which seemed to be negatived by the eclipse of 1869.

Cool hydrogen exists.

I think, if we had merely determined that there was this cool hydrogen, all our labour would not have been in vain, as it shows the rapid reduction of temperature; but there is more behind. I told you that M. Mädler, in summing up the observations made up to 1860, came to the conclusion that part of the corona was certainly solar, and that whether the outer portions were or were not solar, was a matter of doubt. I do not say that we have settled that absolutely, but we have firm evidence that *some* of the light of the corona is due to reflection between the earth and the moon. The outer corona was observed to have a rosy tinge over the prominences, and the spectrum of the prominences was detected many minutes above them, as well as on the dark moon. It could not have got this colour *at the sun*, for its intrinsic colour is green, and the red light of the hydrogen supplied at the sun is abolished altogether, is absorbed, and can only reach the corona *at the sun*, so to speak, as dark light.

Part of the corona is solar.

It is a great fact that we are sure, as far as observation can make us sure, that there is a glare round the hydrogen, which gives us the spectrum of hot hydrogen on the corona, *where we know that hot hydrogen does not exist.*[1] Assume the hot hydrogen which gives us the red light to be only two minutes high, the spectroscope has picked it up eight minutes from the sun? The region of cool hydrogen is exaggerated in the same way. We get it where there is no indication of the cool hydrogen existing. And then with regard to the element which gives us the line of the green, we get that twenty minutes or twenty-five minutes away from the sun. Well, no man who knows anything about the matter will affirm that it is certain that the element exists at that distance from the sun.

Part is due to glare of different layers of solar atmosphere.

Therefore I think we have absolutely established the fact that as the sun—the uneclipsed sun—gives us a glare round it, so each layer of the chromosphere gives us a glare round it. That is exactly what was to be expected, and that it is true is strengthened by the observation—a most important observation made in Spain—that the air, the cloud, every thing between us and the dark moon, are the same spectrum that we got from the prominences themselves.

Given, however, the layers and elements in the chromosphere extended as far as you will, and apparently increased or not by reflection *not at the sun*, we have still to account for rays, rifts, and the like. If anyone will explain either Mr. Brothers' photograph or Mr. Gilman's picture of the eclipse of 1869, containing those dark bands starting from the moon and fading away into space, and the bright variously-coloured rays between them, on any solar theory, he will render great service to science. But in the meantime I must fall back upon M. Mädler's opinion of 1860, with the addition to it that I have stated, that we have found, at all events, *that some* of the doubtful light is non-solar; we have turned the opinion into a fact.

Bear in mind that close to the sun you have a white layer composed of vapours of many substances, including all the outer ones; outside this is a yellow region; above that a region of hydrogen, incandescent and red at the base, cooler, and therefore blue, higher up, the red and blue commingling and giving us violet; and then another element thinning out and giving us green. Take these colours in connection with those which are thrown on our landscapes or on the sea during eclipses, each region being lit up in turns with varying, more or less monochromatic light, and that light of the very colour composing the various layers, each layer being, as I have shown, so much brighter than the outer ones that its light predominates over them: is it too much to suggest to those who may be anxious to attempt to elucidate this subject, that probably, if they would consider all the conditions of the problem presented by that great screen, the moon, allowing each of these layers by turn to throw its light earthwards—the inequalities of the edge of the *globular* moon allowing here light to pass from a richer region, here stopping light from even the dimmer ones—they would be able to explain the rays, their colours, variations, apparent twistings, and change of side? I do not hesitate to ask this question, because it is a difficult one to answer, since the whole question is one of enormous difficulty. But difficult though it be, I trust I have shown you that we are on the right track, and that in spite of our bad weather, the observations made by the English and American Government Eclipse Expeditions of 1870 have largely increased our knowledge.

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[1] We learnt in 1871 that hydrogen *does* exist there! (1873.)

Effect of the inequalities of the moon.

With increase of knowledge generally comes a necessity for changing the nomenclature belonging to a time when it was imperfect. The researches to which I have drawn your attention form no exception to this rule. A few years ago our science was satisfied with the terms *prominences*, *sierra*, and *corona*, to represent the phenomena I have brought before you, the nature of both being absolutely unknown, as is indicated by the fact that the term *sierra* was employed, and aptly so, when it was imagined the prominences might be solar mountains! We now know many of the constituent materials of these strange things; we know that we are dealing with the exterior

Question of nomenclature.

portion of the solar atmosphere, and a large knowledge of solar meteorology is already acquired, which shows us the whole mechanism of these prominences. But we also know that part of the corona is not at the sun at all. Hence the terms *leucosphere* and *halo* have been suggested to designate in the one case the regions where the general radiation, owing to a reduced pressure and temperature, is no longer subordinate to the selective radiation, and, in the other, that part of the corona which is non-solar. Neither of these terms is apt, nor is either necessary. All purposes will be served if the term *corona* be retained as a name for the exterior region, including the rays, rifts, and the like, about which doubt still exists, though it is now *proved* that some part is *non-solar;* while for the undoubted solar portion the term *Chromosphere*—the bright-line region—as it was defined in this theatre now two years ago, exactly expresses its characteristic features, and differentiates it from the photosphere and the associated portion of the solar atmosphere.

Here my discourse would end, if it were not incumbent on me to state how grateful I feel to Her Majesty's Government for giving us the opportunity of going to the eclipse; to place on record the pleasure we all felt in being so closely associated in our work with the distinguished American astronomers who from first to last aided us greatly; and to express our great gratitude to all sorts of new friends whom we found wherever we went, and who welcomed us as if they had known us from our childhood.

THE ATMOSPHERE OF THE SUN.[1]

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IT is now as nearly as may be two centuries ago since a man, whose immortal fame has made of Cambridge a place of pilgrimage, proved to the world by "reason and experiments" that "the light of the sun consists of rays differently refrangible;" and in the same book in which the results of his work are recorded, we find the following pregnant question, which shows that he who had thus, all unknowingly, given us the means of discovering the truth, had already, at one bound, arrived at a conception of it which I propose to show in the present discourse is right in the main.

Newton's eleventh Query.

"Are not," said Newton, in his eleventh Query, "the sun and fix'd stars, great earths vehemently hot, whose heat is conserved by the greatness of the bodies, and the mutual action and reaction between them, and the light which they emit; and whose parts are kept from fuming away, not only by their fixity, but also by the vast weight and density of the atmospheres incumbent upon them, and very strongly compressing them, and condensing the vapours and exhalations which arise from them?"

It should be a lesson to those high in place, who have it in their power to encourage a widening of the boundaries of knowledge, and fail to do so, to reflect that the world had to wait for considerably more than a century for the next steps which brought Newton's work with the prism, and his Query, into closer connection, although the prism was competent to lead us to a discovery of as high an order of importance in one branch of physical astronomy as gravitation is in another.

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[1] The Rede Lecture, 1871, delivered in the Senate House, Cambridge, on May 24, 1871.

Wollaston's and Fraunhofer's observations.

The steps I refer to were Wollaston's detection of the dark lines in the solar spectrum, and Fraunhofer's observations of the spectra of some of the fixed stars in which similar lines were detected.

Here, then, we had the sun and stars linked together by still another bond—the spectra of both contained dark lines; but the opinion of the time was, that far from being great earths vehemently hot, they were great earths cool and habitable as our own.

Still another half-century was sacrificed to our imperfect organization in matters of science, and then the mystery of these dark lines was solved—solved in part, that is to say; and by a man whom this ancient University also claims, and one who is still among you to receive the honour which is his due—I mean Professor Stokes—although the discovery, which is the most important one of our age, is generally ascribed to Kirchhoff and Bunsen, as they were the first to publish it.

Now, I need not tell you that the first obvious outcome of this splendid generalization is the proof that the sun and fixed stars are undoubtedly "vehemently hot;" for if there is anything at all in spectrum analysis which teaches us or can teach us anything about the sun or stars, we proceed on the assumption that we have in the sun and stars "vehement heat;" and the beauty of the reference to the "great earths" is further enhanced by the fact that, instead of finding the sun composed of substances which are unknown to us, many elements which are most familiar to us earth-dwellers are found there.

That the sun and stars were great earths vehemently hot was now, in fact, put beyond all question by the prism: for I need scarcely remark that not only were the dark lines revealed by that instrument in the solar spectrum now shown to be due to the absorption, by *incandescent gases and vapours*, of light proceeding from *something more intensely heated still;* but the presence of hydrogen gas and the vapours of sodium, iron, magnesium, barium, and many other terrestrial elements in the atmosphere, was absolutely demonstrated by the exact fitting of some of the dark lines I have referred to with bright ones in the spectra of the vapours of those elements. The sun and stars burst into song, and the whole heaven was filled with new and exquisite harmonies.

The sun and stars are earths vehemently hot.

But Newton refers not only to a great heat, but to a "fixity" of the "parts;" by which reference, I take it, he shows us that he perfectly understood that the heat of the fixed stars and of the sun could not be the heat of combustion, arising from chemical change, but the heat of incandescence.

Kirchhoff's theory.

In the theory of the sun based by Kirchhoff on his experiments, the general surface of the sun—the photosphere—is regarded as liquid, as one unbroken molten sea, "in continual motion, as our own are when raised by storms and foaming waves;" and these waves are ascribed to the enormous changes of temperature occurring in an overlying solar atmosphere, and the force of the currents which must in consequence be produced. The atmosphere, relatively cooler than this molten sea, is on this theory the corona, a phenomenon visible to us during total eclipses; the most effective absorbing layer being situate at some distance above the sun. The only "condensation" which Kirchhoff allows is that rendered evident to us by spots, which according to him were clouds suspended in the atmosphere, and therefore situated above the photosphere, on which the atmosphere, according to him, rested.

Now Newton—as I hold is clearly evident from the latter part of his Query—had in his mind something very different from this—something which I hope to show is more in harmony with all the telescopic evidence, and with some recent work with that instrument of his, the prism, in which the attack on the sun has been varied from that hitherto employed.

Introduction of the analysing spectroscope.

In the researches which culminated in the discovery to which I have alluded, ordinary solar light was admitted into the slit of the spectroscope irrespective of the part of the sun whence it came; the light even might have been reflected from a cloud. In this way it is clear we deal with the average of all the light radiated from all parts of the sun, assuming that different parts of the sun do give us light of different kinds. But if by means of a telescope we first obtain an image of the sun, and so arrange the spectroscope that only the light of the portion of the sun the image of which is on the slit can enter it, then it is clear that we shall be able to examine the spectrum of any portion of the sun, whether facula or spot or general surface, and thus detect differences, if they exist, in the spectra of these different regions.

This method, of course, during eclipses enables us to study spectroscopically the various phenomena surrounding the sun; and by employing many prisms, we can, to a certain extent, continue this study even without an eclipse, since the vapours exterior to the photosphere give us bright lines which defy dispersion and remain visible as lines after we have dispersed into invisibility the ordinary atmospheric illumination near the sun's place.

This premised, let me state broadly some of the results bearing on the nature of the sun's atmosphere already obtained by this method, both during eclipses and at other times, connecting them as far as may be with ordinary eye and telescopic observations.

First, generally as to the region above the photosphere where variously coloured phenomena are seen during

The chromosphere is distinguished by bright lines.

eclipses—a region which has been named the Chromosphere on this account, to distinguish it from the white photosphere. With the new method employed *without an eclipse*, we find bright lines of different height in the spectrum visible all round the sun; and although the spectrum is generally much more simple than we should have supposed, I have thus seen hundreds of lines reversed. There is generally no part of the sun's periphery where they are absent; the heights of the various lines observed change very slowly at times in sweeping over a long arc, at others rapidly in sweeping over a short one,—over a region, in fact, where there is a prominence. The shorter lines are generally visible only where the higher lines are highest, and are most numerous when the highest lines are brightest.

Now, what substances do these lines indicate?

The longest lines due to hydrogen.

The longest lines are due to hydrogen, as proved by the fact that they are prolongations of the Fraunhofer lines C, F, one near G, and *h*, in the solar spectrum. The next highest line, one in the orange, corresponds with no absorption line in the spectrum, and there can be little doubt that it represents a new element. The heights of the other lines with corresponding Fraunhofer lines, when visible in the same prominence, are almost invariably in the following order:—

Magnesium or sodium, nickel or barium, and iron follow.

Magnesium or Sodium, highest.
Nickel or Barium, next below.
Iron and other lines, shortest.

But there are other lines thus visible at times, which approach the sodium level, and these lines are either bright in the solar spectrum itself, or have no corresponding Fraunhofer lines. Of this more presently.

The number of the lines, and their height and brilliancy, evidently depend upon some action coming from below, from the region of greater heat.

Now, in eclipses our knowledge is carried further, because we are face to face with the phenomena, and no longer have to pierce the bright atmospheric veil by which these regions are generally hidden.

The hydrogen lines vary in height.

In the first place, the hydrogen lines are no longer highest, nor are they all of equal height; and in the next, the number of lines visible is increased; both of which effects might be expected to follow from the improved conditions of observation.

It is rendered evident, then, by these observations with the *spectroscope*, that we are here dealing with *layers* due to the thinning out of the different vapours: such layers, as Mr. Johnstone Stoney has insisted upon and which Professor Pierce has shown, must exist in a mass of mixed vapours, so situated, under the joint action of diffusion and gravity. Now this is exactly the evidence furnished to our eclipse observers by the *telescope*. They have recorded nearest to the sun's limb a white region, the many-lined layer in the spectroscope, next (going outwards) a yellow layer. Here we have lost most of the lines, and have only the substance which gives the yellow line associated with hydrogen and the unknown exterior substance. Next to this, and generally mixed with it in prominences, *i.e.* disturbed regions, we have the layer in which, having dropped the substance which gives the yellow line, we have only hydrogen and the outside substance: this is the red layer. Outside this is a violet region, indicating the mixture of sub-incandescent hydrogen with the exterior substance; and last of all we lose even the relatively cool hydrogen, and have one substance giving us one line in the green, and possibly one or two fainter lines in the yellow. The exact extension of these layers above the photosphere has not yet been determined.

The outer layers give only a green line.

It was in reference to these layers that Mädler, in summing up the results of the eclipse work up to 1860, remarked: "We cannot share the doubts of those who are afraid to surround the sun with too many envelopes."

Still, granting all these layers, it is clear from what I have stated that neither by the new method of spectro-

When large numbers of lines are reversed, the absorbing layer is not high up.

scopic observation nor during eclipses do we easily and obviously see all the dark Fraunhofer lines reversed; and when they are reversed in great numbers, the tell-tale lines indicate that the absorbing vapours which give rise to them do not extend to any great height. So that, in spite of the power which the spectroscope gives us, we do not observe all the lines round the sun bright, which we get dark in the light coming from his disc. Does not this mean that we are not dealing with the whole of the solar atmosphere? Let us consider this point. First, a difficulty of observation may come in the way: the atmospheric light which we have to deal with in one case, and the associated continuous spectrum in another, may be too strong for us; or, again, it may be that the vapour which gives us the majority of the Fraunhofer lines is so limited that our instruments are not capable of picking up all that is going on. However this may be, the fact remains that we do not *easily* account for all the Fraunhofer lines; and the prism tells us that the further we go from the sun the less chance have we of finding them, since, when we get to the extreme regions of the corona, the spectrum, instead of being complicated, is one of extreme simplicity. In fact, the chances are that the spectrum of that region only consists of one faint line, with a glimmer of continuous spectrum. Hence, then, we must work nearer the sun in our search for the reversal of the outstanding Fraunhofer lines.

Now, on Kirchhoff's hypothesis the photosphere is a molten sea. If that were so, we should have no atmosphere below its surface, and the surface would be so level that we must find the atmosphere above it, which we do not readily. But let us assume that the surface of the photosphere is not level; then there is a possibility in favour of some such notion as this—that the photosphere, being cloudy or gaseous, has an uneven surface, with faculous ridges and domes, interspersed with cavities, which perhaps are indicated by what are known to astronomers under the name of *pores*. This I maintain is in harmony with tele-

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scopic observation; and if this be the state of affairs, it may be that most of the absorption lines which we get in the spectrum, which we cannot account for in the bright lines of the spectrum of the regions above the photosphere, may be due to the absorption which is going on in these places.

The solar spectrum is an average spectrum.

And here permit me to remind you that the solar spectrum with which we are all so familiar is not the spectrum of any one part of the sun; it is an average spectrum,—a spectrum built up of all the spectra obtained by an examination of every point of the sun.

It must not be imagined that every part of the sun will give us such a spectrum as this. It does not; and here we get great help in the point we are considering.

What facts, then, have we to guide us in this matter? Let me place before you one or two.

Different behaviour of substances when incandescent.

What is seen when the spectroscope, instead of being occupied with the light outside the sun's limb, is dealing with the light on different portions of his disc?—when in fact we are observing the vapours between us and the sun. Now under these conditions, as they have a hotter substance behind them, they act as absorbers of the light; whereas when they have no hotter substance behind them, as is the case when we see them outside the sun's limb, they act as radiators, so that instead of dealing with bright lines we are now dealing with dark ones. If you place the slit of a spectroscope on any part of the sun you choose to examine, you will find invariably that in a disturbed region you will get, along the whole length of the spectrum and parallel to its length, darkish bands of more or less intensity and fineness.

What does this mean? It means, first, that there is some difference in the general absorption of the light proceeding from the portions of the sun of ordinary brilliancy, and the darker portions. It shows that what a spot is to the sun in general, so is a pore to each small portion of the solar surface. If we were nearer the sun, we should probably find that each small portion of the sun's surface

The spectrum is brighter in the regions of the faculæ.

would, in a powerful telescope, give us *en petit* what we now get *en grand*. These darkish bands running along the spectrum are due to small spots or pores. Where we observe a faculous region in the same way, instead of these darkish lines we get, as it were, bands of brightness, something very much brighter than the spectrum of the ordinary photosphere.

Selective absorption in the spots.

We will now go a step further, so that another difference which exists may also come out in its strongest form. Let us observe a spot of some considerable dimensions. Here the indication of the continuous absorption which darkens the spectrum throughout its length is more obvious still. I shall be able, I hope, subsequently to show you that this is a very valuable indication of particular conditions of this region; but we have now merely to note the phenomena. To do this, let us limit our attention to the double line D, the two dark lines indicating the lines of absorption due to the vapour of sodium. Imagine that on the slit of the spectroscope lies the image of the spot: we see that, in addition to the general absorption which is always to be found in spots, and which is absent when we are dealing with a spotless portion of the sun, we have a considerable thickening of the absorption lines, which, in the case I have taken, is due to the vapour of sodium.

I might have taken other parts of the spectrum, containing the iron and calcium lines, and the same phenomenon would have been observed. Here, then, we have this chain of facts:—

(*a*) The spectrum of a facula is brighter than the spectrum of the ordinary solar surface.

(*b*) Hence the ordinary solar spectrum has undergone a general absorption.

(*c*) The general absorption increases gradually and continuously as the prism analyses the light of a spot with shelving sides, until it is greatest in the spectrum of the umbra.

(*d*) These variations in the general absorption are accompanied by variations in the selective absorption, the thickness of the lines varying continuously with the amount of general absorption.

The denser vapours do not as a rule visibly rise into the chromosphere.

(*e*) In the chromosphere observed at the limb we do not generally see the spectral lines of the denser vapours. In observations of spots, on the contrary, the indications of increased selective absorption are generally limited to these vapours.

Now, it is not necessary for me to establish that spots are below the ordinary level of the general surface of the sun: I shall take this for granted. What then follows from the chain of facts which I have ventured to bring before you? This, namely, that the vast atmosphere to which Newton referred, and about which we are now inquiring, must certainly not rest on the surface of any such thing as a liquid sea, as Kirchhoff proposed, but is carried down continuously by these new researches to the bottom of the deepest spot. We do not know how deep these spots are, but it is clear that we must have a considerable atmosphere to deal with below the upper level of the photosphere. Hence the photosphere must be a something suspended in that atmosphere—a conclusion, I hold, which is drawn equally from telescopic observations.

Let me refer to these observations. One of the most admirable drawings of a sun-spot we owe to Father Secchi, of Rome. This gives us the best idea of a spot as seen in a telescope. The story told is of stupendous forces at work in a material which answers readily to those forces; and a cloudy structure is probably the one which could best give us a result anything like that observed.

Mr. Rutherfurd's exquisite photographs of a series of changes in the same spot give us a like result, with the advantage that this is a story told by the sun himself. In these photographs we see the same spot enlarging and changing from day to day as it passes over the sun. Now it has become double, and again it appears to be breaking up altogether; always giving evidence of the most tremendous convulsions in the photospheric region. Therefore, in addition to the spectroscopic evidence, we have the actual evidence of the appearance, under a high telescopic power, of changes which can hardly go on except in a material mobile to a great degree. Still, enormous as these changes are, I shall show you that they are as nothing compared with the changes that go on in the higher regions of the atmosphere—the regions above the photospheric layer.

The telescope confirms the spectroscope.

Let me give an instance, and refer to two drawings of the same prominence, made at an interval of ten minutes. When I tell you the size of the prominence, and the time the change took to register itself, it will be obvious to you that we are dealing with a region still more capable of re-echoing to the slightest force. The prominence to which I refer was 40,000 miles high. In the interval of ten minutes the forces at work were such, that at the expiration of that time scarcely a trace of its original structure was left. Even these changes are not the greatest that I might refer to among my own observations, and, indeed, those of others; for, fortunately, spectroscopes are getting more common than they were, and observers in all lands are getting rich harvests of facts, and among them abundant evidence of the point I am now enforcing—namely, that the motions are most rapid and the changes most intense in those parts of the atmosphere where its constitution is simplest: in the photosphere we have marked changes from day to day; in the chromosphere from hour to hour—I had almost said from minute to minute.

As we descend the atmosphere, then, we find the number of its constituent materials to increase, the general absorption to become more apparent, the lines to thicken, and the action to be less rapid.

Further, we find the most perfect continuity wherever it is possible to observe it. Hence we are justified in concluding from the evidence of the chromosphere a complete continuity of the solar atmosphere from that portion of it

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surrounding the sun, which gives us thin bright lines, down to the lowest depth of the spots, where the same lines are observed to be inordinately thickened.

The study of the spots will add to our knowledge of the lower strata.

This brings me to the end of what I have to say on this part of my subject—a part of it full of hope; for if we study the spectra of all spots as carefully as the spectrum of the general surface of the sun has been studied, if we look out for the deepest spots, and if it should so happen, as we have already grounds for supposing, that at a particular point in the sun-spot period the spots are deeper than they are at other times, and if we are careful to work most diligently, then we shall be able to carry our records of the solar atmosphere very much lower, and discover lines which are not included in our observations up to the present time.

We may already guess in what direction discovery will lead us. Dealing with known elements, we have outside all invariably hydrogen; then, next, a layer of magnesium and sodium; lower still, of iron, calcium, and so on. If we take these facts in connection with the other one, that we have not the heaviest metals writing their record in the solar spectrum, may it not be that the reason is that the vapours of those metals are so far beneath their lighter brethren that they are never thrown above the photosphere by which the spectrum is reversed?

Newton suggests exhalations.

Newton, in his Query, suggests not only "vapours," but "exhalations." Of the vapours we have already heard something; the lines in the solar spectrum are the result of the existence of them, but what do we know about these exhalations, in which term we get a dynamic notion introduced? Do they exist? and if so, is the exhalation gentle like the upsoaring of our own clouds, or fierce and terrible, as we might easily imagine it to be in such a surrounding?

How runs the record on this point?

We have two methods of determining this. We can first establish whether the prominences as seen on the limb soar upwards, and, if so, with what velocity.

Motion indicated by a change in the position of spectral lines.

But I venture to think there is a little doubt connected with this method of grappling with the subject. I question whether we have here the best way of determining the presence and movements of the exhalations, but we have a very undoubted one, which I need not demonstrate in this Senate House, which depends upon this consideration: if we have any of the vapours on the sun moving with a velocity comparable with the velocity of light, the selective radiation and absorption of those vapours will indicate the motion. In other words:—their absorption and radiation when in motion will not be the same as their absorption and radiation when in a condition of rest.

The spectroscope has enabled us to come to the facts in all these cases; but in order to bring them entirely satisfactorily to your notice, it will be necessary to say a few words about the language in which the records are written.

Such changes of wave-length give rise to what, in the case of the prominences, I have called motion-forms.

Those are not the forms of the prominences themselves which we have on the slit, but forms due to the changes of refrangibility, which deflect the bright or dark line under examination now towards the violet, now towards the red, and now towards both: both on and off the sun they shiver the perfect image of a perfect slit, now into hard crooked lines, and again into most delicate films.

Difference between changes of wave-length on the disc and limb.

It is clear that when such observations as these are made at the sun's edge, we are not dealing with Newton's exhalations, because any motion on the edge of the sun must be a sort of wind-motion on the sun. But the same facts are to be gathered from the centre of the sun itself, where of course any change of wave-length must be due to motion in the direction of the line of sight, and must therefore be an indication of an up or down rush. Now on the sun we have this further evidence in our favour, that almost invariably when the line due to any vapour is deflected towards the violet, that vapour no longer plays the part of an absorber; it radiates and appears bright, and generally somewhere in each region of disturbance we get the hydrogen lines bearing on them somewhere near the bright region, what I have called a *lozenge.* These lozenges are in fact generally in connection with the root of a prominence seen upon the sun. As a rule, when we get the bright line indication of an uprush, we get an absorption line by the side of it, often moved towards the red, which shows that we have relatively cooler hydrogen descending from above the disturbed part, but at times the index line in many cases disappears altogether; that is, we have no longer relatively cooler hydrogen—the whole of the superincumbent hydrogen has been heated to the same temperature as that of the newly-ejected material, which is as hot or at times hotter than the photosphere. This being so, we have still another argument for the continuity of the solar atmosphere to the regions below the niveau of the photosphere, where we must look for Newton's dense atmosphere, and not above it, and here is proof also that the deeper we go the hotter we get.

Indication of an uprush on the centre of the disc.

There is still another point to which I should like to call attention for a moment. We often find something very like the lozenge again in connection with small spots; and I would also call attention to the disappearance of the hydrogen dark lines, in connection with its appearance, and the way in which in small spots we get the hotter hydrogen rising up.

Again, we may get this lozenge associated with a complete brightening of the hydrogen near it, which *remains at rest,* which is proved by the fact that the neighbouring portion of the absorption line more or less suddenly brightens without changing its wave-length. Here we have a sporadic welling-up of hydrogen and other substances, giving us their lines bright in the spectrum, and lines *constantly bright* in the spectrum are probably due to a *constant* welling-up of other, as yet unknown, materials.

I claim to have shown that Newton, when he spoke of the condensation of vapours, the presence of exhalations, and the vast weight of the atmosphere, was utterly right, so far as modern science can endorse his opinion.

Bearing of the laboratory on the observatory work.

I must now come to another part of my subject, slightly different from what has occupied our attention hitherto. I refer now to the work done in the laboratory and not in the observatory; and I trust you will admit that the future study of the physics of the sun will be the combined work of the astronomer, the physicist, and the chemist.

The different lines we see in our instruments when we examine the solar prominences are not all alike. Some of the lines vary very much from the appearance of the C line of hydrogen for instance. In fact, in one line, the F line, we get a trumpet-shaped appearance. The line widens as it approaches the sun, so that it resembles an arrow-head, resting on the thin absorption line which forms the shaft. This is not only true of the hydrogen lines, but of the lines due to the injection of other substances into the chromosphere.

Now, the question is how to determine experimentally to what this widening of the F line is due?

After Kirchhoff's announcement of spectrum analysis, one of the great points connected with it was that the spectrum for each substance was absolutely distinct, and it was generally regarded as invariable, so that—given the spectrum—you could make no mistake whatever about the element, the vapour or the gas which gave off this spectrum.

Spectrum analysis can indicate the physical state of substances.

We have, however, got further than this now, and we may say that certain conditions of each substance give us certain spectra. So that it may be said that spectrum analysis is competent not only to indicate the substance, but also somewhat of the physical conditions in which that substance is existing at the time it is giving us its spectrum.

Plücker and Hittorff were the first to give us an idea of this extension; they held that for certain gases and vapours which they examined there was a spectrum of the first order, a spectrum of the second order, and so on; but the cause of these various spectra was not stated with any certainty.

Changes in spectra are not due to temperature per se.

I have been fortunate enough to be associated with Dr. Frankland in a continuation of the work of Plücker and Hittorff; and in connection with the mere astronomical results to which I have referred, we, as a result of a three years' inquiry, have come to the conclusion that there is a definite reason for certain changes in spectra; and that these changes are not due to temperature *per se*, but to pressure. We find, for instance, that by increasing the pressure, say of hydrogen, we thicken out the lines, and especially the F line, exactly as it is thickened out in the lower region of the chromosphere.

A Sprengel pump and induction coil enable us easily to recognize the wonderful changes in the colour of hydrogen as its pressure is varied, and the addition of a small spectroscope shows us the wonderful changes in the thickening of the different lines which accompany these changing colours. Since by varying the pressure of hydrogen we can thus vary the thickness of the lines, it is possible to observe the spectrum of hydrogen in a tube, and to place the spectrum we get from the hydrogen in the sun, side by side with the spectrum we obtain from the hydrogen in the tube. We can vary the pressure of the hydrogen in the tube, so that its spectrum exactly fits, so to speak, the spectrum of the hydrogen in the sun; and hence we are enabled to determine the pressure of the hydrogen at the sun. And this, no doubt, will some day be done; but the thing is not quite so easy as this. There are more elements than hydrogen in that part of the sun which so conveniently gives us these bright lines; and we have in all questions of pressure not only to take into account the actual pressure of the hydrogen, but the combined pressure, so to speak, of all the vapours which exist in that stratum; and so we have a very great inquiry before us before a more approximate estimate of the pressure can be definitely stated than the one we have already given.

Demonstration of the tenuity of the upper atmosphere.

We can easily establish the extreme tenuity of the atmosphere in the upper regions of the sun's atmosphere, which are visible to us in eclipses, by such an experiment as the following one:—I have in this globe some hydrogen, extremely attenuated, though perhaps not so attenuated as the hydrogen in the confines of the sun's atmosphere, but it so nearly approaches it, that when we examine the light produced by the passage of a spark by means of the spectroscope, we see exactly the same hydrogen spectrum that eclipse observers tell us they get from the exterior portion of the sun's atmosphere. Most of you are under the impression that hydrogen is a red gas. The prominences of the sun are red, and built up of hydrogen; but the hydrogen in this globe is not red at all, it is green.

By such methods as these,—by determining first of all the conditions which change the lines from thick into thin, or thin into thick—then noting the exact thickness of the lines we wish to match—then changing our pressure and fitting it as well as may be so as to represent the same phenomena artificially,—we introduce a new method of inquiry into solar physics of the most tremendous power.

Let me take another instance. By sealing a piece of sodium in a tube with a rare atmosphere of hydrogen, and heating the sodium so as to fill the tube with layers of sodium vapour, which are denser and denser as we approach the source of the vapour, Dr. Frankland and myself have shown that when we allow a beam of light to traverse the tube, the absorption of the sodium vapour depends upon the density we choose to give to it, and that the thickness of the absorption line varies with the density of the vapour. In fact, we can thus artificially reproduce the exact thickness observed in the D lines, when they are thinnest in a high prominence and thickest in a deep spot.

The amount of absorption depends on the density of the vapour.

Here again, I think, you will agree that we have abundant proof of the continuity of the solar atmosphere.

The sun, in fact, has not only taught us how to attack him in this way, but in these experiments we have foreshadowed a new science altogether, a *Quantitative Spectrum Analysis*.

A quantitative spectrum analysis.

A reference to another experiment will make this more clear. When the vapour of magnesium is injected into the prominences, we get the well-known triple line *b*, showing us three bright lines beyond the sun's limb. But the lines are not of equal height. I explained it in this way. While in the case of the F line reduced pressure thins the line, in the case of *b* reduced pressure not only thins all the lines, but abstracts one of them from the spectrum altogether. To test this explanation Dr. Frankland and myself prepared a tube in which the electrodes were composed of magnesium, and we observed the spectrum of the magnesium vapour between the poles while the pressure of the hydrogen in the tube was gradually reduced. Judge of our delight when we found that after a little time the third line began to disappear; and that when the pressure was still further reduced, the line vanished entirely.

This at once explained what I had seen on the sun, but this was not all. Knowing that both in the chromosphere and in our experimental tube the magnesium vapour was associated with hydrogen, it was an interesting inquiry to see how the spark would behave when taken in air without the intervention of any tube.

By means of a new method of experimentation in which the spark was treated as the sun, and made to throw its image on the slit, the appearances observed on the sun were reproduced exactly by the spark. The nearly pure magnesium vapour close to the pole gave us the three lines, while further away, when the vapour was mixed with air, we got the spectrum due to reduced pressure; in fact, the reduced quantity of the vapour, so to speak, gave the same effect as the reduced pressure in the other experiment.

It is easy to show also that with increased pressure comes such an increased brilliancy as we get in the sun, where going inwards we have first the faint corona, then the hydrogen layers visible only in an eclipse, then those observed every day by the new method, and last of all the photosphere itself.

Experiments.

Here is a simple experiment bearing on this point: I have in this tube some hydrogen at a very low pressure, and at the bottom of it some mercury. So long as the mercury is cool the spark passes through nearly pure hydrogen, and the tube is lighted up with only a faint glimmer, the equivalent of the auroral discharge. But if, instead of having our mercury vapour almost absent from the tube, in consequence of the low temperature of the liquid supply at bottom, we drive this liquid mercury at the bottom of the tube into a state of vapour, we find that we not only change the colour of the discharge in the lower regions of the tube, but imitate what we get on the sun itself. As the discharge passes through the denser layers, and renders them incandescent as they are at the sun, the brilliancy increases enormously as the source of the vapour is approached, and in each stratum of different density of the mixed gas and vapour in the tube we have the same increase of brightness as we observe in the similar strata of the sun's atmosphere.

Prominences over spots.

Although the laboratory work to which I have alluded is necessarily of such a kind that the fact that the changes to which I have referred are due to pressure and not to temperature may be considered by some to be still open, I think that a phenomenon of not unfrequent occurrence which presents to us a bright line prominence on the sun's disc flitting high over a spot, the bright lines of sodium in the prominence being thin, and the dark lines in the spot immoderately thick, may be accepted as final, for *we must assume that the prominence is hotter than the spot* which lies beyond it, or otherwise its lines should not be bright, and this being so, it cannot be an increase of temperature solely which thickens the lines.

This a fruitful field of inquiry.

If, then, we get these indications, namely, the thickening of the lines, the increased brilliancy of the different layers and of the different vapours, vapours observed both here and on the sun; if we get all these changes reproduced in our laboratories, and can get at the causes of them, it is not too much to hope that if such work be assiduously and continuously carried on, in course of time we shall arrive, spectroscopically, at a profound insight into the nature of solar phenomena, while at the same time the spectroscope has added an enormous new field of observation in the possibility it has afforded us of daily chronicling the positions, dimensions, and materials of the prominences, which are at once the most beautiful and most delicate indications of the forces at work in our central luminary. And I need not stop to point out that our central luminary is not alone in question. All the knowledge we can ever hope to gain of the physical constitution of those distant orbs, which illuminate what are to us the distant realms of space, must be got by a study of solar physics. The beautiful researches of Carrington, De la Rue, Stewart, and Loewy, have established that our sun is a variable star. Among the work to be done now is the grouping of the stars into classes, taking the spectra of a facula, of the general surface of the sun, and of a spot as the types.

Birth of a new Science.

We, in fact, are now witnessing the birth of a new science, the cultivation of which seems capable of widening the horizon of our knowledge in a way we could scarcely have dared to hope for. But it is a science which requires costly apparatus and appliances above the means of most individuals, and a continuity of thoroughly organized work extending over centuries, which we can never hope for as the result of private enterprise.

May we expect the State of this enlightened country to foster such a work as this? Our Government, although its motto is *ex luce lucellum*, will probably decline, though I feel certain it is a work which other Governments will take up; we are driven then to our great corporate bodies, and among them especially to our ancient Universities, as almost our only hope in our own land, until the time comes when the true and best duties of a Government are known.

And why should not Cambridge take up the work which now languishes for want of help? Surely, in doing so, she would only be following up her ancient precedents, would be in entire sympathy with her past.

I do not dare to make a more urgent appeal, but I do venture to express a hope, that when some one, perhaps in the long future, and in this Senate House, tells a succeeding generation how the many secrets of the sun have been read, he shall be able to refer to a physical observatory at Cambridge, as I to-day have referred to Newton's prism.

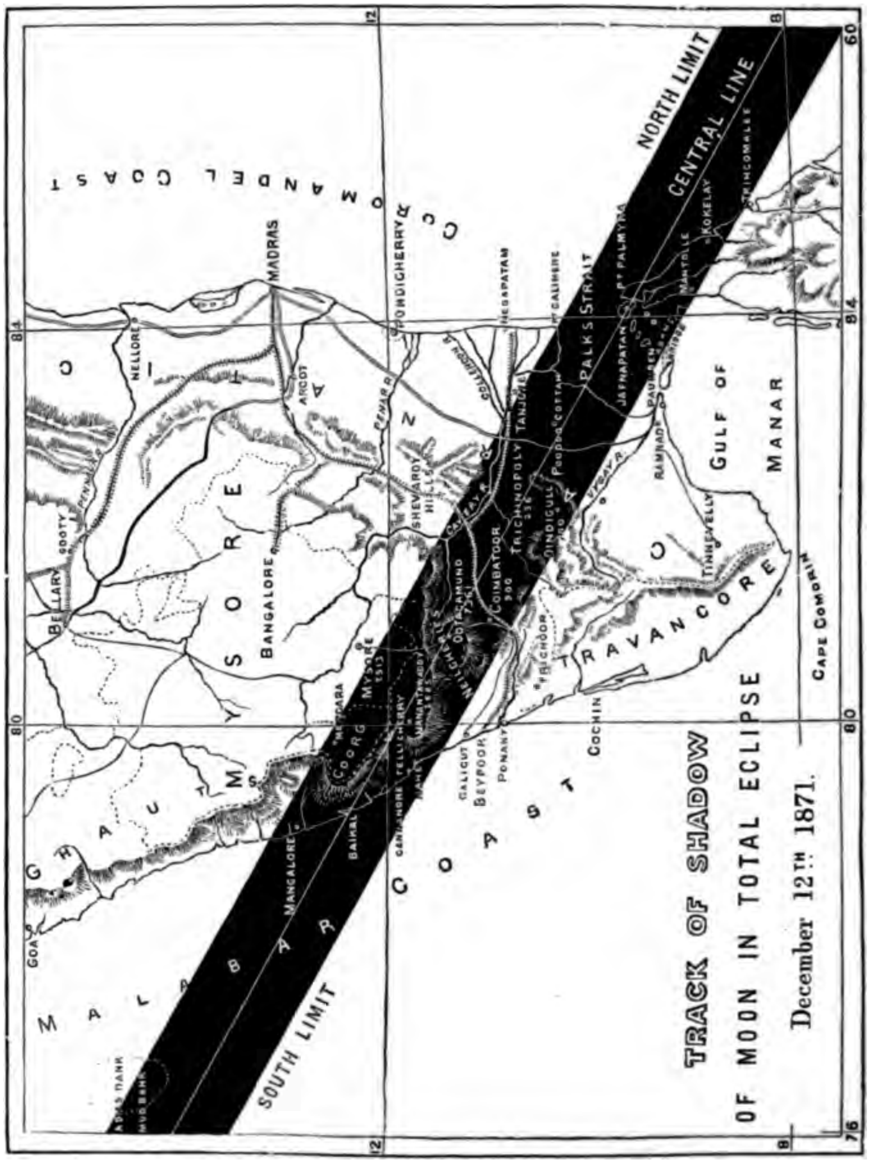
THE ENGLISH ECLIPSE EXPEDITION, 1871.

I.—PRELIMINARY.

CHAP. XX.

Regions where eclipse was total.

AS will be seen from the accompanying map, the central line of this eclipse of the sun in 1871 first met the earth's surface in the Arabian Sea, and, entering on the western coast of Hindustan, passed right across one of the most important parts of that country in a S.E. by E. direction. In this part of the peninsula, the sun was about 20° above the horizon when totally obscured. The duration of totality was two minutes and a quarter, and the breadth of the shadow about seventy miles. On leaving the eastern coast of the Madras Presidency, the central line crossed Palk's Straits, passing about ten miles S.W. of the island Jaffnapatan, and over the northern part of Ceylon, where the small towns of Moeletivoe and Kokeley lay near the central line, and the well-known naval station of Trincomalee, about fifteen miles to the S.W. Continuing its course over the Bay of Bengal, the shadow crossed the S.E. point of Sumatra, and touched the south-western coast of Java, where Batavia, the capital, was nearly sixty miles N.E. of the central line; and two other smaller towns, Chidamur and Nagara, were also very near the middle of the shadow path. In the Admiralty Gulf, on the N.W. coast of Australia, the eclipsed sun was only ten degrees past the meridian, and not far from the zenith: in consequence of which the totality lasted



TRACK OF SHADOW OF MOON IN TOTAL ECLIPSE December 12th 1871.

FIG. 120.

four minutes eighteen seconds, or only four seconds less than the time of greatest duration. Lastly, passing through the most barren and uninhabited portion of Australia, and crossing the Gulf of Carpentaria and the York Peninsula, the shadow ultimately left the earth's surface in the Pacific Ocean.

Weather chances.

At the outset not too much was known about the chances of weather at any place; but what was known seemed to point to a fair chance of success in both India and Ceylon, as the eclipse occurred during the monsoon, but in any case the experience of the last expedition showed that for such a momentary phenomenon these chances need not be taken too seriously into consideration, seeing that then where the finest weather was predicted a terrible pall of cloud covered the sky.

Before I proceed to give the results of this expedition, it is necessary that I should state as briefly as possible how it came to be undertaken, and the plan of operations proposed.

In 1871 I was on the Council of the Royal Astronomical Society, and I naturally expected that that body would take some steps in the matter; feeling at the same time that as I was perhaps the youngest member of it, the proposal to take these steps should not come from me. When I found, however, that all the business at the last available meeting (that in June) had been concluded, I hesitated no longer, and brought forward a motion on the subject.

First proposals.

A committee was appointed, and I was invited to take charge of a spectroscopic expedition. This offer I declined, as I was strongly of opinion that the camera was as important an aid as the spectroscope, and I was convinced that a stronger party than the one proposed by the Astronomical Society was essential. I therefore drew the attention of the Royal Society to the importance of utilizing the coming eclipse. I give the following extract from the communication I addressed to that body:—

Letter to the Royal Society.

"In my opinion the fundamental points of attack are:

"*a.* Spectroscopic observations made with such an instrument as the one I took out to Sicily, *equatorially mounted*, and with reference spectra.

"*β.* Photographic observations made with such an instrument as the one I took out to Sicily; namely, a camera with large aperture and small focal length, *equatorially mounted.*

"Perhaps I may clear the ground by stating what, in my opinion, is comparatively UNIMPORTANT, so far as the crucial points are concerned, though to be tolerated if the crucial points are strongly taken up.

"*a.* Photographing prominences.

"*β.* Sketching anything but the *changes* in the corona.

"*γ.* Polariscopic observations.

"*δ.* Observing Baily's Beads.

"There should be one instrument, and Mr. Pogson could probably provide this in India, to determine the position of prominences before and after totality. *During totality they should not be observed at all except incidentally.*

"At each place (*i.e.* India, Ceylon, Australia) the spectroscopes should be employed for half an hour (to be on the safe side) before totality, in scrutinizing the crescent at its narrowest place and the chromosphere outside the following limb of the moon.

"At each place, as before defined, there should be a spectroscope with a finder, and equatorial motion (or some equivalent arrangement) directed to the sun's centre, to record any changes which take place in the spectrum from, say, half an hour before to half an hour after totality, and during totality, *bien entendu.* The relative darkness or brightness of the lines should be recorded every ten seconds.

"This spectroscope should have moderate dispersion, large object-glasses for collimator and telescope, and with focal length such that two or three degrees round the sun

CHAP. XX. should be taken in (*i.e.* 1° or 1½° from the sun's centre), and a large field.

"To come to the details of the expedition to Ceylon; I am of opinion that it need not exceed the following numbers, as my Sicilian experience has taught me that we may depend upon much valuable help from the officers at the place of observation:—

"1 Telescope-Spectroscopic observer; 2 assistants.

"1 Photographer; 2 assistants. This duty perhaps may be entrusted to skilled Sappers.

"1 Spectroscopic observer; 1 assistant, or 8 in all.

"Among general observations, I would point out as being of extreme importance:—

"α. Rays before, during, and after totality—their length, direction, and colour.

"β. Colours of the various layers of chromosphere, and of clouds and landscape. The *order* of these colours is of great importance.

"γ. Dark rays or *rifts;* whether they change, and whether they extend to the dark moon, or stop short above the denser layers of the chromosphere.

"δ. The colours of the corona between bright or dark rays.

"ε. All changes in corona.

"ζ. Comparative brightness of rays and chromosphere and outer corona." . . . . .

Action of Royal Society.

The Council of the Royal Society at once took energetic action; instruments were sent off to Australia, and all promised well, when owing to events which I need not further particularize the efforts of the two learned bodies entirely collapsed. Public attention, however, had been called to the importance of an expedition, and the withdrawal of the Astronomical Society from all participation exercised no prejudicial influence whatever, for at the meeting of the British Association at Edinburgh, the

President, Sir William Thomson, in his opening address, after referring to the recent sun work, went on to say:—

Extract from Sir W. Thomson's address.

"During six or eight precious minutes of time, spectroscopes have been applied to the solar atmosphere and to the corona seen round the dark disc of the moon eclipsing the sun. Some of the wonderful results of such observations, made in India on the occasion of the eclipse of August 1868, were described by Professor Stokes in a previous address. Valuable results have, through the liberal assistance given by the British and American Governments, been obtained also from the total eclipse of last December, notwithstanding a generally unfavourable condition of weather. It seems to have been proved that at least some sensible part of the light of the 'corona' is a terrestrial atmospheric halo or dispersive reflection of the glowing hydrogen and 'helium' round the sun. I believe I may say on the present occasion, *when preparation must again be made to utilize a Total Eclipse of the sun, that the British Association confidently trusts to our Government exercising the same wise liberality as heretofore in the interests of science.*"

This expression of opinion was at once followed up: at a general meeting of the Association it was resolved to ask for a sum of 2,000*l.* in aid of the proposed expedition. This was granted the day after the application was made.

Work of committee.

A committee was at once appointed. Instructions were prepared and a communication addressed to the Directors of the Peninsular and Oriental Steam Navigation Company, who entered so warmly into the scheme that the reduced terms they offered amounted really to a magnificent private endowment of so liberal a nature that the Committee resolved that the Expedition should consist of twelve persons. Invitations were accordingly sent to Janssen, Young, Ångström, Zöllner, Respighi, and Peters, among foreign men of science, and to a large number in the three kingdoms who were known to be interested in solar or spectroscopic research, begging them to take part in the work. Many of those thus addressed were unable to devote the time necessary for the long journey; but eventually a party, the names of which will be detailed further on, sailed on the 26th of October, in the *Mirzapore*.

While these arrangements were going on in England, the Government by means of the telegraph communicated with the Indian and Ceylon authorities in order that the Expedition on reaching Pont de Galle might be forwarded without delay to the observing stations in the north of Ceylon, and some points not finally determined upon on the mainland of India,—an addition to the original programme brought about by a complete discussion of the weather conditions by the Committee.

THE ENGLISH ECLIPSE EXPEDITION, 1871

(*continued*).

II.—THE BEKUL PARTY.

CHAP. XXI.

FORTUNATELY for the Expedition, General Selby, who takes great interest in science, who is an admirable artist, and who had observed the Eclipse of 1868, had already thrown the whole weight of his position into the arrangements, and sent tents to Bekul. He now offered guards for the various parties, stated his intention to issue an order calling for volunteers among the officers at Cannanore and Mangalore, and expressed his intention of coming to Bekul himself to take part in the work. The light-heartedness, therefore, with which the remaining four of the Expedition—Dr. Thomson, Captain Maclear, Mr. Davis, and myself—saw the anchor come out of the turbid water and steam got up for the last final run[1] into the jungle, may be imagined. *Bekul.* Bekul had been a mystery; nobody knew anything about Bekul; the word was printed remarkably small on most maps: what was it like? Trincomalee was a naval station; Jaffna a large and important town; Poodocottah the residence of a Rajah; Manantoddy the head-quarters of all kinds of new industries, chinchona, coffee, teak, and the like, with any number of English, and a club to boot: but Bekul! We should at all events be looked after.

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[1] We were in the flag-ship, H.M.S. *Glasgow*, to the captain of which—Captain Jones—the Expedition can never express sufficient gratitude. The final run here referred to was from Cannanore.

No wonder the good ship *Glasgow* would have steamed right past the place if Mr. Lewis M'Ivor, the indefatigable Assistant Collector, and Mr. Pringle, the representative of the Public Works Department, who had been sent to meet us, had not been on the look-out, and come off in a small boat, waving a large flag, so energetically that the ship's head was turned slightly towards the shore, and then, as the men in the chains gradually got their song down to "Quarter less six," we saw a ruined fort, with crowds of natives, and a solitary house among the cocoa-nut trees.

This was Bekul. The natives were not natives of the place—for, practically speaking, there was no place—but men who had come many miles to see the great fireship that was to bring the astrologers, and they were now feasting their great eyes with the unaccustomed sight. There was more excitement for them to follow. When Mr. M'Ivor had made all arrangements for landing in the morning, he went ashore, and, as a measure of precaution, ordered the fire which had been lit to mark his landing-place to be kept in. In doing this the sun-scorched grass in the vicinity was soon ablaze, and looked so like an illumination in honour of the ship's arrival that a blue light was burnt, a rocket sent up, and a gun fired, to the intense delight of the natives, who fully appreciated the impromptu illumination.

Impromptu illumination.

In the morning, on landing, which was a most ticklish operation in the surf, work began in earnest with the rising sun. The spots chosen by Mr. M'Ivor for the temporary observatories for the telescopes—in old Tippoo's fort, which commanded the whole horizon, and a vacant space near for the photographic work—were inspected and found so satisfactory that the instruments immediately on landing were taken there, unpacked, and before long were approximately in their positions. While this was going on the *Glasgow* went to prize firing, an operation which had been delayed while the Expedition were on board, for fear of damaging instruments and breaking object-glasses. This tremendous proceeding on the part of the ship, and the wonderful similitude of the telescopes to the native idea of a big gun, soon wrought a wonderful change in the ideas of the dwellers along the coast. The Eclipse was a pretence. There was war! If otherwise, why the firing with shot? Why occupy the fort? Why erect big guns in the most commanding place in its, to them, vast extent? Why these soldiers from Cannanore? Instant action! All high-caste women and all gold into the interior; men still to watch the action of the "gods," and if possible probe their motives and intentions to the very bottom. Of course the only way of meeting this rumour, which might have proved very unfortunate to us, was to allow everybody free access to the observatories. When the natives found that the big guns were made of very thin metal, and that the biggest of them when looked boldly into showed merely the face of the inspector considerably enlarged, all fear passed away, and probably the women were recalled, but on this point I have no certain knowledge.

Alarm of natives.

After the inspection of the fort the party marched down to the encampment about half a mile to the north and along the shore in a shady nook. In the centre was the little bungalow long disused, but now done up for the occasion; round it—leaving, however, the sea-view open—were arranged the tents. The Collector of South Canara (Mr. A. M'C. Webster) and his able assistant (Mr. M'Ivor) had made every possible arrangement; and when I say that this included necessarily the bringing of our bread some forty miles every other day or so, some idea of what had to be done may be gathered. Few seemed to know the capabilities of the place in the local production of those articles, such as cheetahs, snakes, &c., for which India is so celebrated. Trying to sleep, therefore, in a hammock

The bungalow and its surroundings.

in the bungalow, with all doors and windows opened, as is the custom here, with rare and curious insects and creeping things evidently all round, the trees and jungle alive with sounds of all pitches and a good many amplitudes, is not always a successful operation for the beginner, but he learns apace. The long pent-up howl of the jackal in the morning soon becomes familiar; scorpions are looked upon as mere nuisances; and so on. As it was, only one alligator, one snake, and one scorpion made their appearance while we were at Bekul. The first and second gracefully withdrew, the latter was safely bottled.

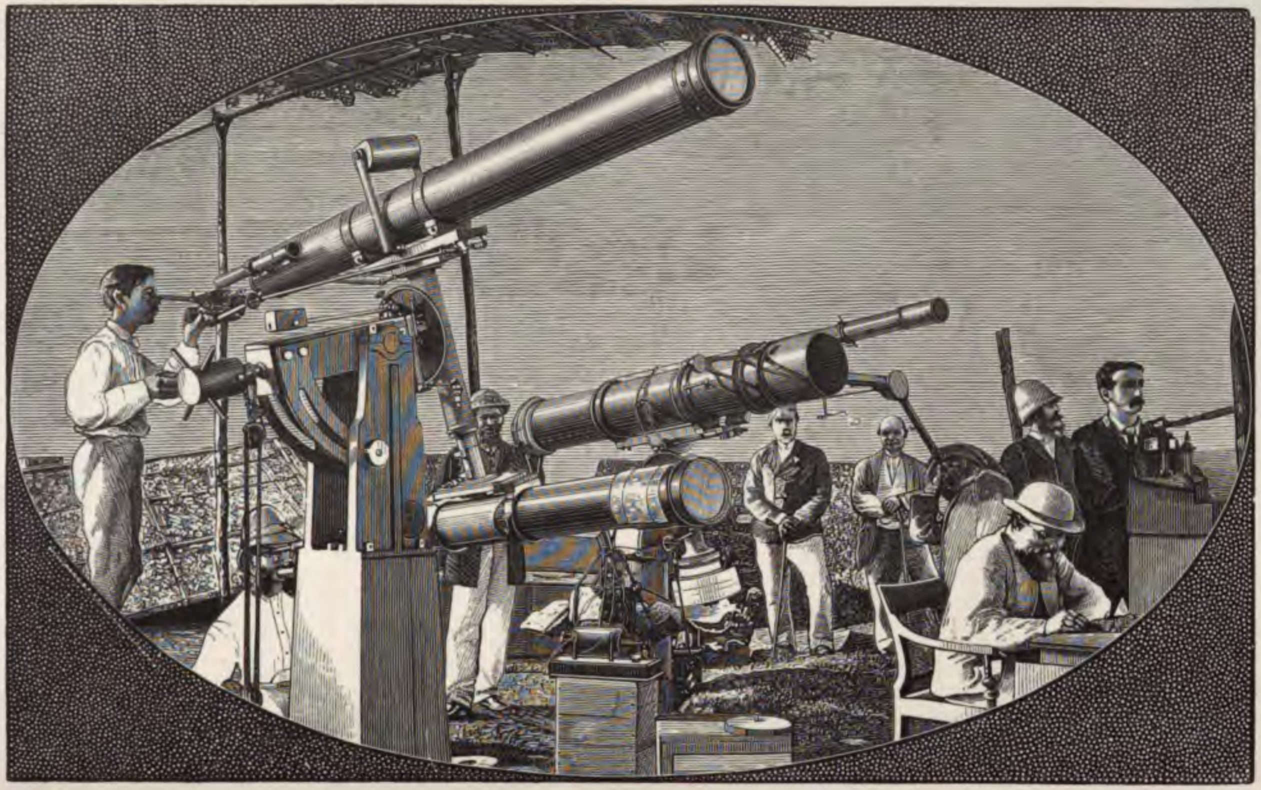
Work in camp.

Let me now say a word about our work in the camp. All rose at 4.30 or thereabouts, then tea and a walk to the observatories to work before the heat of the day began. By nine we all had had quite enough, and, indeed, some of the party got fever for a day or so by exceeding this limit. Then home, a bath and breakfast, a rest and then tiffin. After this, observatory again from about four till eight for adjustments. Then back to the camp for dinner and rest. By keeping strictly to this programme, the instruments were all ready at the critical moment, though, owing to the necessity of making each instrument do double duty, the adjustments were not absolutely perfect. Adjusting a telescope against time, at home, is no easy work; but in the jungle, with two telescopes on each stand, a minimum of tools, and no labour specially skilled in this work, though any amount of willing help—Mr. Pringle and his assistant, Mr. Fernandez, being a host in themselves—it was a work of tremendous difficulty.

The eclipse.

Now for the eclipse. In spite of the warnings against the West coast, the weather was magnificent up to the evening before the long-prepared-for event, when clouds came on, and the dinner-party in the bungalow—which now numbered some twenty people, General Selby and his Brigade-Major, Colonel Farewell, Judge Walhouse, Capt. Christie the Superintendent of Police, and others having joined the camp—was slightly uneasy. On rising at 4.30

Pl. IV. To face *p.* 343.



THE OBSERVATORY AT BEKUL.

on the memorable day, however, it was found that the clouds had all disappeared during the night, with the exception of a low bank to seaward which did not alarm us; so the whole party repaired to the observatories in the best of spirits.

The observing party.

I must content myself with describing what went on in the fort. Imagine a round, rough, embattled platform, some thirty or thirty-five feet in diameter, with two large telescopes placed nearly in the middle, pointing to the rising sun. At one of them, a large 9¼ reflector, was stationed myself; at the other Captain Maclear and Mr. Pringle. Close to the reflector is a table with some instruments upon it, at which are sitting two persons—one Captain Bailey, who is to tell how the time is going on; the other a native employed in the Collector's office, to take down anything that is said, his paper being carefully marked, so that the place of his record denotes the time of the observation. By the table is standing Mr. M'Ivor, whose duty it was to assist me in opening the slit of the spectroscope, if that should be required, and the like. Mr. Fernandez is there, too, to watch the clock, and disconnect the telescope from it at the right moment. Captain Christie is acting as amanuensis for Captain Maclear. In the other corner, at tables, are General Selby, Colonel Farewell, and Judge Walhouse, with cards and telescopes before them, ready to sketch the corona. This is the attacking party, and there are police to keep out intruders.

First contact.

As the first contact took place at Bekul a few minutes after sunrise, when the image of the sun was unsteady, the exact time could not be noted, but that was a small matter. Slowly the eclipse crept on; swarms of small Rajahs, squires, and natives of all sorts and conditions, rapidly coming up in their manchiels, and taking up their position round the fort, which they are not permitted to enter.

Rahoo

There is strict silence in the fort, and the work of recording the comparatively unimportant phenomena visible in telescope, spectroscope, and polariscope, before totality, goes on like clockwork; but it is very different below. The natives see in the eclipse their favourite god devoured by the monster Rahoo, and they, like the observers, are not there for nothing. Yells, moans, and hideous lamentations rend the air as the monster seems to them to get the upper hand; the excitement increases, and evidently something is afoot. Mr. M'Ivor's sharp eye detected an intended sacrifice of fire, the intended fuel being the long, parched grass covering the landscape exactly between the fort and the eclipsed sun. In a moment he pointed this out to Capt. Christie; in a few more a posse of police was stamping out the flames, and the smoke-bank, which threatened to upset all the work, gradually died away; the moans, however, still continued, and Rahoo worked its wicked will.

The corona.

It is now time to return to the fort. Captain Maclear announces bright lines in abundance. I had barely time to observe these, whisper a word or two, and reach the large spectroscope, before I exclaimed "Steady!" the signal agreed on for commencing the counting of time. Instantly everybody in the fort heard Captain Bailey's clear voice ringing out, "You have 120 seconds," and there in the leaden-coloured, utterly cloudless sky, shone out the eclipsed sun! a worthy sight for gods and men. There, rigid in the heavens, was what struck everybody as a decoration, one that Emperors might fight for; a thousand times more brilliant even than the Star of India, where we then were! a picture of surpassing loveliness, and giving one the idea of serenity among all the activity that was going on below; shining with a sheen as of silver essence; built up of rays almost symmetrically arranged round a bright ring above and below, with a marked absence of them right and left, the rays being composed of sharp radial lines, separated by furrows of markedly less brilliancy.

The observers, however, saw little of this glorious sight. Now, at the spectroscope we got the words "Hydrogen

Prism train and polariscope.

spectrum, 1474 longer than F." Following close upon Mr. M'Ivor's command, "Polariscope," we got the words "Polarization vertical over everything; strong:" this was the verdict of the Savart. Next the biquartz came into play. "Yellow and brown, with green on both sides, faintly radial," was next heard. Then from Captain Bailey, "You have eighty seconds more." This was the signal for observing the eclipsed sun through a long train of prisms, an attack in which I placed great confidence, and which I then rushed to make. "Four circles, 1474 same size [as the rest][1] and faint," was at once written down by the amanuensis. Then another manœuvre. "You have still thirty seconds more," said Captain Bailey. In a moment Captain Maclear and myself changed instruments—I to observe the structure of the corona with the Astronomer-Royal's 6-inch telescope, Captain Maclear to note the spectrum of any part which I might feed him with, in a spectroscope of large dispersion mounted on my own refractor of slightly greater aperture.

Structure of corona.

"Definite structure five minutes from sun," "No spectrum," "Structure like [that of the great nebula in] Orion," "No spectrum," were now uttered antiphonically by myself and Captain Maclear in rapid succession, and the eclipse was soon over to the outsiders, apparently before its time; but not to me at the refractor. "Structure still visible." "Still visible," "Still visible," now went on for nearly three minutes, and then the corona vanished into thin air.

Other observers.

In the fort Captain Maclear, and below, Dr. Thomson and Mr. Davis, did noble work; and far away at Jaffna, Trincomalee, Poodocottah, and Manantoddy, were others all working out the programme; while at Ootacamund and Avenashi were Janssen, Tennant, Herschel, Pogson, Hennessy, and a host of others, strung up to the same point of tension. Of the results of the work of course as yet I can say nothing, except that at Bekul Dr.

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[1] I have bracketed the words omitted for brevity's sake at the time.

Thomson's polariscope results agree exactly with my own; and that, thanks to the skill of Mr. Davis and his friendly assistants, five exquisite photographs rewarded Lord Lindsay's endowment.[1]

At eight Dr. Thomson and myself, not yet recovered from a slight attack of fever—the sure result of unrest here—were being rapidly carried in manchiels on the backs of a host of bearers, to gain intelligence of the other parties and to confer with Janssen, Tennant, and Pogson. Little rest was there to be got along the jungle road, what with the plaintive, rhythmic moaning of the bearers, the flashing of the torches in the dark night, the crossing of innumerable fords, and, above all, the great question, "What have the other parties done?" still unsolved.

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[1] This refers to the fact that the photographic arrangements at Bekul, which formed one of the fundamental parts of the work to be done, were conducted partly at Lord Lindsay's expense by his assistant Mr. Davis, who formed part of the British Association Expedition.

THE ENGLISH ECLIPSE EXPEDITION, 1871

(*continued*).

III.—A LETTER FROM OOTACAMUND.

CH. XXII.

SURELY if eclipse expeditions had their mottoes, that of the expedition of this year should be *per mare per terram;* for it has been *per mare per terram* in our case with a vengeance! Probably when we return, the curious individuals who total up in the *Times* the aggregate number of years those people have lived whose deaths are there recorded, will, in asking us for our autographs, beg also a detailed statement of the number of miles each of us has travelled in the performance of our duty. I fear it will be very difficult to give the information; and if the temperature in the shade be wanted too, the thing will be perfectly hopeless: for, thank goodness, we took the precaution to bring no thermometers; had we done so and looked at them, it might have been all over with us. Let me point my remarks. A week ago I was at Bekul, having travelled I know not how many thousand miles by sea, and having scarcely set foot on land for a month. We were in the jungle, the heat was burning, some of us had fever, and it was opium which enabled me at all events to get through the day of the eclipse, for it was that memorable day just a week ago. Since then, by night and by day, Dr. Thomson, Captain Maclear, and myself have been—

Indian travelling.

I seek a word; *wafted* is too weak, *jolted* is too strong, for some parts of our journey, though ridiculously lacking in expression for others—well, *conveyed* from Bekul, now in men-carried conveyances, the cunning bearers with their plaintive, by no means unmelodious moaning, keeping step, giving us an idea of the tremendous labour they were undergoing, and reminding us of a certain journey which we must all make once; now on men's shoulders; now in bullock bandy, speed about two miles an hour, thanks to a brutal breach of contract, which has upset my plans terribly; now in Indian railway carriages, average speed ten miles an hour, temperature of carriage at noon unknown; and lastly in the horse transit of the Madras Carrying Company. Oh that their carriages were as good as their arrangements and the speed of their horses! And now, here I am shivering, surrounded by hoar-frost, with a *soupçon* of a difficulty of breathing in this higher air after the dense atmosphere of the jungles, but all the same in an earthly paradise with hedges of roses, although it is mid-winter, the whole place a perfect garden. I am at Ootacamund, at an elevation of some 7,000 feet, with an Australian fauna around me; and within a few hours I hope to see Janssen, who is still here; Tennant, Herschel, and Hennessy I have unfortunately missed, owing to the breach of contract already referred to.

General success.

We can all of us, or nearly all of us, afford to laugh now at any inconveniences we have suffered; for of the eleven who landed at Galle nine have seen the eclipse, some of us perhaps as an eclipse has never been seen before. Unfortunately, to the regret of all, Mr. Abbay and Mr. Friswell, who were among the best prepared for doing good work, and were at a station at which everybody said cloudless weather was certain, found themselves on the 12th in a storm of cloud and mist, which obscured the sun for, I believe, the whole day. With this exception the telegrams from all the English parties have been sent regularly; while we have all been thankful to learn from the telegrams which Dr. Janssen and Colonel Tennant have had the great courtesy to send me, that they too saw the eclipse well, as also did Mr. Pogson, as I gather from the newspapers; but of course the details of their observations are still unknown to me. Hence, I can only give the facts observed by the party at Bekul and Poodocottah; Prof. Respighi, who observed at that station, having joined me at Pothanore, the station on the Madras Railway, at the foot of the hills which we ascended yesterday from 4.30 A.M. till 1 P.M.

Noble aid.

But before I say a word about the observations themselves, it is incumbent upon me to express our deep obligations to the Supreme, Madras, and Ceylon Governments for the magnificent manner in which they have aided us. Nothing could be more complete than the arrangements at Bekul made by the Collector, Mr. Webster, and his assistant, Mr. McIvor, both for the work to be done and the comfort of those who had to do it. The same must be said for the Poodocottah party, where not only the Collector, Mr. Whiteside, but the Rajah did everything in their power, the latter loading the observers with presents when they left. We have at present heard only of the discomforts of the Manantoddy party, and it is clear that here the local arrangements were in strong contrast to those elsewhere. The Ceylon parties, who parted from the main body at Galle, have doubtless been well looked after: as Captain Fyers, the Surveyor-General of the island, accompanied and aided them in their observations.

This brings us to another part of the arrangements. The Ceylon party had the unreserved use of the Government steamer the *Serendib*, to take them from Galle to their places of observation, Jaffna and Trincomalee, both on the coast, and the accommodation on board was perfect. The Indian parties proceeded to their various destinations, or the ports on the coast nearest to them, in the Admiral's flag-ship the *Glasgow*, which, however, could not remain to bring them back—a circumstance which has given rise to very considerable inconvenience and great risk for the instruments, which are now scattered all along the line, to be sent to the coast and from the coast to Bombay or Galle, as circumstances may determine. This of course was not to be helped, and we must hope for the best, especially as all the parties have done their utmost in superintending their repacking, and handing them over in perfect condition to the different Government officers who accompanied each party. Still, although it was not to be avoided, the withdrawal of the ship has been the unfortunate circumstance in the arrangements. Nothing could exceed the kindness of the Admiral, who vacated his own quarters to give us room; of Captain Jones, who took the warmest interest in our proceedings, and helped the arrangements greatly; and of the officers of the ship generally. Without the equal kindness of Mr. Webster at Bekul, the step from the Admiral's cabin into the jungle hut would have been a seven-league one.

Stations and personnel of the parties.

I will at once state the general arrangements of the parties, and what I at present know of the observations. The stations and observers as finally arranged were as follows:—

Bekul—Analysing Spectroscope, Captain Maclear and Mr. Pringle. Polariscope, Dr. Thomson. Photography, Mr. Davis.

Manantoddy—Analysing Spectroscope, Mr. Friswell. Integrating Spectroscope, Mr. Abbay.

Poodocottah—Spectroscope, Professor Respighi. Sketches of Corona, Mr. Holiday.

Jaffna—Integrating Spectroscope, Capt. Fyers. Polariscope, Captain Tupman and Mr. Lewis. Photography, Captain Hogg.

Trincomalee — Spectroscope, Mr. Moseley and Mr. Ferguson.

Besides these observers, we had at Bekul the valuable assistance of General Selby, commanding the troops in Canara and Malabar (for whose help in supplying guards' tents, &c., the friends of Science cannot be too thankful), Colonel Farewell, Judge Walhouse, and others, in sketching the corona. At all stations, of course, most precious help in various ways was given by all present who volunteered for the various duties, though some of them lost a sight of the eclipse in consequence. Among those who helped in this way at Bekul were Mr. M'Ivor, Mr. Pringle, Captain Bailey, who timed the eclipse, Mr. Cherry, and Capt. Christie, the Superintendent of Police, whose presence there turned out to be of the most serious value ; for the natives, seeing in the eclipse the great monster Rahoo devouring one of their most sacred divinities, not only howled and moaned in the most tremendous manner, but set fire to the grass between our telescopes and the sun, to propitiate the representative of the infernal gods. Captain Christie with his posse of police stopped this sacrifice at the right moment, and no harm was done.

Detail of observations.

Now for the observations. Perhaps I may be permitted to begin with my own, as at the present moment I know most about them. I determined to limit my spectroscopic observations to the spectrum of a streamer and to Young's stratum, thereby liberating a number of seconds which would enable me to determine the structure of the undoubted corona with a large refractor ; to observe the whole phenomena with the naked eye, and through a train of prisms with neither telescope nor collimator ; and finally with a Savart and biquartz. I found the 120 seconds gave me ample time for all this, but owing to a defect in the counterpoising of my large reflector, which disturbed the rate of my clock, I missed the observation of the bright line stratum (assuming its existence) at the first contact. At the last contact Mr. Pringle watched for it and saw some lines.

Having missed this, I next took my look at the corona. It was as beautiful as it is possible to imagine anything to be. Strangely weird and unearthly did it look—that strange sign in the heavens! What impressed me most about it, in my momentary glance, was its serenity. I don't know why

I should have got such an idea, but get it I did. There was nothing awful about it, or the landscape generally, for the air was dry and there was not a cloud. Hence there were no ghastly effects, due generally to the monochromatic lights which chase each other over the gloomy earth, no yellow clouds, no seas of blood—the great Indian Ocean almost bathed our feet—no death shadow cast on the faces of men. The whole eclipse was centred in the corona, and there it was, of the purest silvery whiteness. I did not want to see the prominences then, and I did not see them. I saw nothing but the star-like decoration, with its rays arranged almost symmetrically, three above and three below two dark spaces or rifts at the extremities of a horizontal diameter. The rays were built up of innumerable bright lines of different lengths, with more or less dark spaces between them. Near the sun this structure was lost in the brightness of the central ring.

But from this exquisite sight I was compelled to tear myself after a second's gazing. I next tried the spectrum of a streamer above the point at which the sun had disappeared. I got a vivid hydrogen spectrum, with 1474 (I assume the place of this line from prior observations) slightly extended beyond it, but very faint throughout its length compared with what I had anticipated, and thickening downwards, like F. I was, however, astonished at the vividness of the C line, and of the continuous spectrum, for there was no prominence on the slit. I was above their habitat. The spectrum was undoubtedly the spectrum of glowing gas.

I next went to the polariscope, for which instrument I had got Mr. Becker to make me a very time-saving contrivance—a double eye-piece to a small telescope, one containing a Savart and the other a biquartz. In the Savart I saw lines vertical over everything—corona prominences, dark moon, and unoccupied sky. There was no mistake whatever about this observation, for I swept three times across and was astonished at their unbroken-ness. I next tried the biquartz. In this I saw wedges, faintly coloured here and there; a yellowish one here, a brownish one there, with one of green on each side the junction, are all the colours I recollect. Then to the new attack — the simple train of prisms which Professor Young had thought of as well as myself; its principle being that, in the case of particular rays given out by such a thing as the chromosphere, or the sodium vapour in a candle, we shall get images of the thing itself painted in that part of the spectrum which the ray inhabits, so to speak; we shall see an image for each ray, as if the prisms were not there. What I saw was four exquisite rings, with projections where the prominences were. In brightness, C came first, then F, then G, and last of all 1474! Further, the rings were nearly all the same thickness, certainly not more than 2′ high, and they were all enveloped in a line of impure continuous spectrum.

I then returned to the finder of my telescope, a 3¾ inch, and studied the structure of the corona and prominences. One of the five prominences was admirably placed in the middle of the field, and I inspected it well. I was not only charmed with what I saw, but delighted to find that the open-slit method is quite competent to show us prominences well without any eclipse. I felt as if I knew the thing before me well—had hundreds of times seen its exact equivalent as well in London—and went on to the structure of the corona. Scarcely had I done so, however, when the signal was given at which it had been arranged that I was to do this in the 6-inch Greenwich refractor, In this instrument, to which I rushed, for Captain Bailey had just told us that we had "*still* 30 seconds more"—which I heard mentally, though not with my ears, as "*only* 30 seconds more"—the structure of the corona was simply exquisite and strongly developed. I at once exclaimed, "like Orion!" Thousands of interlacing filaments varying in intensity were visible, in fact I saw an extension of the prominence-structure in cooler material. This died out somewhat suddenly some 5′ or 6′ from the sun, I could not determine the height precisely, and then there was nothing: the rays, so definite to the eye, had, I supposed, been drawn into nothingness by the power of the telescope; but the great fact was this, that close to the sun, and even 5′ or 6′ away from the sun, there was nothing like a ray, or any trace of any radial structure whatever to be seen. While these observations were going on, the eclipse terminated for the others, but not for me. For nearly three minutes did the coronal structure impress itself on my retina, until at last it faded away in the rapidly increasing sunlight. I then returned to the Savart, and saw exactly what I had seen during the eclipse, the vertical lines were still visible!

Captain Maclear's well-known skill was amply rewarded. I need only here therefore refer to the extreme value of his observations, adding what I should have stated before, that I saw the bright lines at the cusps, as he was so good as to draw my attention to them. I am, however, not prepared to say that they were visible through a large arc of retreating cusp.

Dr. Thomson confined his observations to the polariscope, using the Savart. He states that his observations were identical with my own.

Mr. Davis's photographic tent was below the cavalier in which our telescopes had been erected; and immediately after the observations I have recorded were over, I went down to see what success had attended his efforts. I was hailed when half-way there with the cheering intelligence "five fine photographs," and so they are, those taken at the beginning and end of the eclipse being wonderfully similar, with, I fancy, slight changes here and there; but on this point I speak with all reserve until they have been examined more carefully than the time at our disposal has permitted, and until they have been compared with those taken at Ootacamund, Avenashi, and, I hope, at Jaffna and Cape Sidmouth.

This exhausts the principal work done by the Bekul party, with the exception of the sketchers with General Selby at their head, who have recorded most marked changes in the form of the outer corona, and Mr. Webster, who was so good as to photograph the eclipse from a fort some eight miles away, with an ordinary camera, and obtained capital results.

Poodocottah.

Next a word about the Poodocottah, the other fortunate Indian party. Professor Respighi has promised to send his results to you with this. About Mr. Holiday's labours I know nothing, except that he has obtained three sketches.

Ceylon.

Concerning the Ceylon parties I give you a verbatim extract from the telegrams. From Jaffna: "Exceedingly strong radial polarisation, 35′ above the prominences; corona undoubtedly solar to that height, and very probably to height of 50′." From Trincomalee Mr. Moseley informs me that he carefully watched for Young's bright line stratum, and did not see it, and that 1474 was observed higher than the other lines.

This is the sum total of the information which has at present reached me. It is clear there are discordances as well as agreements, the former being undoubtedly as valuable as the latter. It remains now to obtain particulars of all the observations of all the parties, before a final account can be rendered of the eclipsed sun of 1871. This, of course, will be a work of months; but if all goes well, I trust to obtain information shortly of the outlines of the work done by the Indian observers and M. Janssen, as I am now remaining in India for that purpose. In the meantime I hope the good people at home will think we have done our duty, and that all the members of the Government Eclipse Expedition of 1871 will soon be safely with them to give an account of their work.

Ootacamund, Dec. 19, 1871.

THE ENGLISH ECLIPSE EXPEDITION, 1871

(*continued*).

IV.—INTELLIGENCE FROM THE OTHER PARTIES.

CHAP. XXIII.

IN the former communication written from Ootacamund, I promised another when I was in possession of more information as to the work done, not only by the British Association parties, but by those representing the Indian and French Governments. Let me now endeavour to redeem my promise, seeing that since that communication was penned I have had the happiness of hearing from M. Janssen's own lips an account of what he did; have met Captain Waterhouse, the last representative at Ootacamund of Colonel Tennant's party; have visited Mr. Pogson at Madras, who obligingly gave me an account of the results obtained at Avenashi; and last, but not least, have learnt since my return home that the Jaffna party were successful, not only with the polariscope, but also with the camera and spectroscope.

Captain Waterhouse.

Within a few minutes of the dispatch of my last article I found that Captain Waterhouse, who assisted Mr. Hennessy in exposing the photographic plates taken by Colonel Tennant's party, was still at Ootacamund, and this welcome intelligence was soon followed by Captain Waterhouse himself, who was so good as to bring with him a drawing of one of the photographs; the plates themselves having been taken down the ghaut by Colonel Tennant, with the intention of comparing them at

Pothonore with those taken by Mr. Davis. Unfortunately, as has been already stated, we missed each other, and so an absolute comparison of photographs did not take place; but from the drawing it was evident that in the two series the main form of the corona was the same. The photographs I learned were very sharp and good, and one appreciates their value the more when it is known that only a very little time before they were taken, any success, even a partial one, seemed out of the question, so persistently did cloud and mist hang over Dodabet on the eventful morning. I gathered that the spectroscopic observations had also been successful, and that a continuous spectrum with 1474 had been observed. If more lines than this were not seen, it is easily to be accounted for by the relatively long focal length of the object-glass employed to throw an image of the eclipsed sun on the slit.

Dr. Janssen.

Not until the morning after my interview with Captain Waterhouse did I learn the whereabouts of Dr. Janssen, who from a study of the habits of the clouds and their prevailing drift, had concluded that the neighbourhood of Ootacamund was not the best that could be chosen. He had consequently taken his departure, and it seemed at first as if his whereabouts was known to no one. At last, however, Professor Respighi and myself came upon his spoor; he was at Sholoor, on the N.E. flank of the range, at the solitary house of a tea-planter, to which there was no road, but which might be reached on ponies if a guide to it could be found. This guide Captain Sargeant, of the Revenue Department, obligingly provided, and in no very long time we reached the beautiful spot which Dr. Janssen had chosen. It will be better that I should state his results in his own words. In a letter[1] to Professor De la Rive, dated December 26, he thus writes:—

"J'ai été favorisé par un ciel d'une pureté presque absolue. Cette circonstance, et surtout les dispositions

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[1] "Bibliothèque Universelle," January 15, 1872, p. 103.

Janssen's conclusion.

optiques toutes nouvelles que j'avais prises, m'ont permis de faire sur la couronne des constatations qui démontrent son origine solaire (pour la meilleure partie).

"Dans mon télescope,[1] le spectre de la couronne s'est montré non pas continu, mais remarquablement complexe. J'y ai constaté :

"Les raies brillantes du gaz hydrogène qui forme le principal élément des protubérances et de la chromosphère.

"La raie brillante verte déjà signalée aux éclipses de 1869 et 1870, et quelques autres plus faibles.

"Des raies obscures du spectre solaire ordinaire, notamment D. Ces raies sont beaucoup plus difficiles à apercevoir.

"Mes observations prouvent que, indépendamment des matières cosmiques qui doivent exister dans le voisinage du Soleil, il existe autour de cet astre une atmosphère très étendue, excessivement rare, à base d'hydrogène.

"Cette atmosphère, qui forme sans doute la dernière enveloppe gazeuse du Soleil, s'alimente de la matière des protubérances, lancée avec une si grande violence, des entrailles de la photosphère. Mais elle se distingue de la chromosphère et des protubérances, par une densité énormément plus faible, une température moins élevée, et peut-être par la présence de certain gaz différents.

"Il y a donc lieu de distinguer cette nouvelle atmosphère solaire. Je propose de la nommer *atmosphère coronale*, désignation qui rappelle que c'est elle qui produit la meilleure partie des phénomènes lumineux qui ont été désignés jusqu'ici sous le nom de couronne solaire.

"En annonçant ce résultat, je n'oublie pas, quant à moi, tout ce que nous devons aux travaux qui l'ont préparé, notamment ceux des astronomes américains aux éclipses de 1869 et 1870."

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[1] Ce télescope a une ouverture de om. 37, et 1m. 42 seulement de distance focale. Les images y sont de 12 à 16 fois plus lumineuses que dans une lunette astronomique ordinaire. Le spectroscope avait été construit pour utiliser toute cette lumière.

It will be seen that the importance of the brilliancy of the image, so strongly insisted upon by the eclipse Committee in their Instructions, had been fully recognised by Dr. Janssen, whose instrument had more light even than those used by the British parties, who used "Browning-With" reflectors of 9¼ inches aperture, and some 6 feet focus.

Hydrogen.

Although my account, in this place and at this time can only be of the most general character, the coincidence obtained by Janssen, Respighi, and myself on one point may be briefly referred to, namely the distinct proof obtained by each of us that above the most vivid chromospheric layer, and even the prominences, we have hydrogen with its most familiar bright lines, and with much of the "structure" of its spectrum: these proofs being derived not only from the old method of inquiry, but from the new one employed by Professor Respighi and myself.

We spent the remainder of the day at Sholoor in mounting the hill at the back of the house to see the observatory, and to admire the wonderful view of the plains of Mysore, which was visible between a break in the hills; while the immediate neighbourhood, with its waterfalls, massive peaks, rocks here, and patches of wood there, steep ravines and tea-clad valleys, presented us with a scene of perfect beauty.

Mr. Pogson.

Next morning we were away before sunrise on our way to Mr. Pogson, whom we found at the Madras Observatory, preparing to exchange time signals with the Jaffna party. Three photographs were taken by Mr. Pogson at Avenashi, but the instrument used was so different from those used at Bekul and Dodabet (not to mention Jaffna) that it is difficult to institute a comparison in the time at my disposal; but it is not to be doubted that they will be of the highest importance when the general results come to be discussed. Mr. Pogson was assisted in the observations by his son and Mr. Chisholm, the Government architect, who was highly successful in sketching the corona and the eclipse effects upon the landscape.

Jaffna.

Come we last to Jaffna. In my former article I referred only to the polariscope and spectroscope work done there. I have since learned that six photographs were taken with the sister instrument to the one used at Bekul.

The observations, in fact, were a perfect success. The morning was clear and bright, and could not have been finer had any one so wished.

At about six o'clock the party and those who were to assist them began to assemble on the Belfry Bastion in the Fort. Captain Tupman observed with the Polariscope and drew during the eclipse, and was assisted by Captain Varian of the *Serendib* as his time-keeper: Mr. Lewis with his telescope and polariscope was stationed inside the hut, with the photographic party, and Mr. Thwaites, Deputy Queen's Advocate, who was assisted by the carpenter of the *Serendib.* Captain Fyers, R.E., with the spectroscope, had for his assistant Mr. W. S. Murray, Deputy Fiscal; and Captain Hogg, R.E., who presided over the photographic department, was assisted by Mr. Twynam, Government agent, and Mr. J. W. Simpson. By these observers the polariscope results were arrived at, a telegraphic summary of which I quoted in my last communication. Six photographs were taken, being one more than we obtained at Bekul; and in the clockwork-driven integrating spectroscope the reversal of the dark lines was seen at the beginning of totality, and the hydrogen bright lines and 1474 during totality. No information yet about intensities.

Sketches were made by Mr. Foenander, of the Surveyor-General's Department, Colombo; Mr. Pargiter, Assistant Government Agent; Mr. Vine, M.C.E., of the Public Works Department; Mr. Carmichael and Mr. Layard of the O.B.C.

The crowd of natives round the Belfry Bastion was very great; they set up a most hideous howl directly totality commenced, fancying that the end of the world was at hand. They were under the impression that the whole of the Expedition with assistants and all here during the eclipse were going to get into a balloon and off to the sun and not return.

Rahoo again.

General success.

It will thus be seen that the hopes of those interested in the various expeditions of this year have not been disappointed. The composition and structure of a part of the corona have been for ever set at rest, while we have seventeen photographs, taken by instruments of the same power and pattern, to compare with each other—eleven taken at the ends of a base line some 400 miles long, and six at an intermediate elevated point, whereby it was hoped to test the influence of the atmosphere on the observed phenomena. Whether the slight mist will have prevented this or not remains to be proved, but anyhow here is a wealth of records unequalled before, and we may hope to learn much of the outer coronal regions from their comparison, not only *inter se*, but with Mr. Holiday's admirable drawings, showing considerable changes, which have also come to hand.

THE ENGLISH ECLIPSE EXPEDITION, 1871

(*continued*).

V. GENERAL STATEMENT OF THE METHODS, USES AND RESULTS OF THE EXPEDITION.[1]

CHAP. XXIV.

I UNDERSTAND my duty to-night to be to give an account of the observations made, not by all who observed the eclipse of last December, but by the members of the party which went out under the auspices of the British Association, and it is extremely fortunate that nothing more is required of me; first, because most valuable work was done by the other parties, which of itself would require more time to state than I have at my disposal; and secondly, because the amount of material obtained by the members who were sent from England, and by the friends who met them at every point, is so great, that it would be impossible in one discourse to give anything like an exhaustive account of it. Here are some of the records in this portfolio. You will see at once that even for one party I can only make a selection, and I am perfectly aware of the extreme responsibility which attaches to anyone who may venture to make a selection out of such an enormous mass of material as we have collected.

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[1] A lecture delivered at the Royal Institution of Great Britain, on Monday, March 22, 1872. The chief results here detailed, obtained by the expedition have been taken from the *ad interim* Report presented to the British Association Meeting in Brighton. The lecture itself dealt mainly with the methods and instruments employed.

Before I proceed to discuss the work done by the different parties, it will be desirable to give an idea of the arrangements, and for this purpose I have prepared several maps, which will enable you to see what the British Association parties did.

Weaker conditions.

In the first instance I may remark that the weather conditions were somewhat problematical. Another point of great importance was that much of the ground was fortunately occupied, and it was essential when placing the parties to bear these two considerations in mind—the possibility of bad weather, and then the importance of so arranging matters that if some of the observers were clouded out, belonging to our parties, then the story might be continued by other observers.

Here we have a map of India, which gives you a general idea of the path of the shadow during the eclipse. The shadow, you see, strikes India on the western coast, and it runs down in a south-westerly direction, and cuts the northern portion of Ceylon.

Local observers.

When we arrived in India we found that the Indian observers, consisting of those well-known men Tennant, Herschel, Hennessy, Pogson, and others, had determined, from their knowledge of the climatic conditions of India at that time of the year, to occupy the central part of the line, and also a station at a low level; the eminent French physicist M. Janssen taking up his position at the top of the Nielgherries. We were to station ourselves either east or west, or both, of these parties. Whether east or west would depend upon the monsoon, and the great question that was being discussed on our arrival was, Was the monsoon favourable?

I have not time to go into the many interesting points touching the answer to this question; but I may say shortly that what we heard was, that if the weather was likely to be bad on the east side of the hill range, generically called the Ghauts, there was a good chance for anyone occupying a position west of those hills. What

Points chosen.

happened was that we did occupy the positions marked by blue wafers on the map, namely, Bekul on the west coast, Manantoddy on the western slope of the Ghauts, Poodocottah in the eastern plain, and in the island of Ceylon, first, Jaffna; and secondly, Trincomalee.

Such were our arrangements. The parties were stationed along the line of totality. Very different were the arrangements of the Sicilian party of the former year. In Sicily we were compelled to throw ourselves *across* the line of totality in the direction which I have indicated on this map of Sicily.

Work to be done.

Now what was the work we had to do? If you will allow me to refer to two or three results of the former Eclipse expedition, I will endeavour to put them before you without taking up too much of your time.

First point.

One of the most important among the results obtained in the eclipse of 1870 was this: far above the hydrogen which we can see every day without an eclipse—far above the prominences, the spectrum of hydrogen had without doubt been observed by two or three of the American observers, who were more fortunate than we were. Among them Professor Young stated, that the spectrum of hydrogen was observed to a distance of 8′ from the sun; he then adds, "far above any possible hydrogen atmosphere." This is point number one.

Second point.

Another of the points was this: the unknown substance which gives us a line coincident, according to Young, with a line numbered 1474 by Kirchhoff, had been observed by the American observers to a height of 20′ above the limb of the dark moon.

Now, it was a very obvious consideration that if we got a spectrum of hydrogen 8′ from the dark moon, when we thought we knew that the hydrogen at the sun did not really extend more than 10″ beyond the dark moon, there was something at work which had the effect of making it appear very much more extensive than it really was; and it was fair to assume that if this happened in the case of the hydrogen, it might also happen in the case of the unknown stuff which gives us the line 1474.

In support of this view we had one of the few observations which were made in Sicily, in the shape of a drawing of the corona, as seen by Professor Watson, who observed at Carlentini. He saw the corona magnificently; and being furnished with a powerful telescope, he made a most elaborate drawing of it, a rough copy of which I will throw on the screen. You will see at once that we had in this drawing something which seemed to militate against the idea that the 1474 stuff at the sun did exist to a height of 20′. According to Professor Watson the boundary of the real corona was clearly defined, its height being far under that stated.

Third point.

Next, we had another observation of most important bearing on our knowledge of the base of the corona. I refer to the announcement of the observation by Professor Young of a stratum in which all the Fraunhofer lines were reversed. It was asserted that there was undoubtedly a region some 2″ high all round the sun, which reversed for us all the lines which are visible in the solar spectrum. We had in fact in a region close to the photosphere the atmosphere of the sun demanded by Kirchhoff at some distance above the photosphere.

Photography.

Last, not least, we had the photographic evidence. There was in Sicily a photographic station at Syracuse, and the Americans had another in Spain. I now show on the screen a drawing—it is not the photograph itself—but a drawing of a photograph made by the party in Sicily; what we have on this photograph, is a bright region round the dark moon, which is, undoubtedly, solar, but stretching out right away from this, here and there are large masses of faint light, with dark spaces between them, which have been called rifts. Now the question is, Is this outer portion solar?

Having thus brought rapidly before you some of the questions which we had principally to bear in mind, and, if possible, settle (though that is too much to hope for in any one Eclipse Expedition) in the work we had to do in India, I will next bring to your notice some new methods of inquiry which had been proposed, with the object of extending former observations.

Importance of photography.

I may here remark that the Royal Astronomical Society, in the first instance, invited me to take charge of an expedition to India merely to conduct spectroscopic observations; but although this request did me infinite honour, I declined it, because the spectroscope alone, as it had been used before, was, in my opinion, not competent to deal with all the questions then under discussion. I have told you that some of the most eminent American observers had come to the conclusion that the spectrum of hydrogen observed in the last eclipse round the sun, to a height of 8′, was a spectrum of hydrogen "far above any possible hydrogen" at the sun. Hence it was in some way reflected. Now with our ordinary spectroscopic methods it was extremely difficult, and one might say impossible, to determine whether the light which the spectroscope analysed was really reflected or not; and that was the whole question.

It became necessary, therefore, in order to give any approach to hopefulness, to proceed in a somewhat different way in the 1871 expedition; and in order to guard against failure, to supplement such new observations by photographs; and fortunately we were not long in coming to a conclusion that this might be done with some considerable chance of success.

Prism train.

I have here a train of prisms. I will for one moment take one prism out of the train, and we will consider what will happen if we illuminate the slit of the lantern with a monochromatic light and observe it through the prism. If we render sodium vapour incandescent, we know we get a bright yellow image of the slit, due to the vapour of the metallic sodium only giving us yellow light. But why is it that we get a line? Because we always employ a line for the slit. But suppose we vary the inquiry. If, instead of a straight line, we have a crooked line for the slit, then we ought to see a crooked line through the prism. Now, allow me to go one step further: suppose that instead of a line, whether straight or crooked, we have a slit in the shape of a ring, shall we see a ring through the prism? You will see that we shall. And then comes this question: If when we work in the laboratory we examine these various slits, illuminated by these various vapours, why should it not happen that if we observe the corona in the same way, we shall also get a ring built up by each ray of light which the corona gives to us; since we know, from the American observations, that there were bright lines in the spectrum of the corona, as observed by a line slit? In other words, the corona examined by means of a long train of prisms, should give us an image of itself painted by each ray which the corona is competent to radiate towards us.

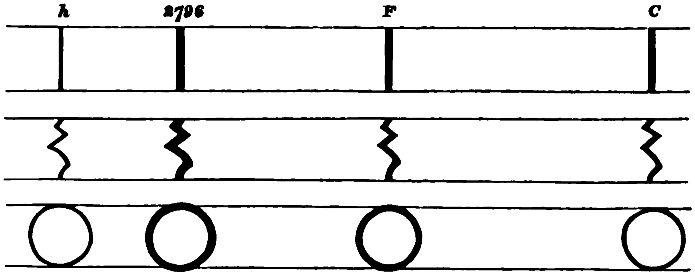

FIG. 121.—The spectrum of hydrogen as seen with a straight, a crooked, and a ring slit.

Shape of slit.

Now let us pass to the screen, the screen merely replacing the retina. We will first begin with the straight slit with which you are familiar—we now have our slit fairly focussed on the screen—we then in the path of the beam interpose one of these prisms, and there we get on the screen a bright line.

Now, to continue the argument, we replace the straight slit by a crooked one, and you see we get a crooked image on the screen. We now replace this crooked slit by a ring. We have now a ring-formed image on the screen. So that you see we can use any kind of narrow aperture we choose, and as long as we are dealing with light which is monochromatic, or nearly so, we get an image of the aperture on the screen.

If we consider the matter further it will be evident that we may employ a mixture of vapours, and extend this result.

Mixed vapours.

We will now, for instance, instead of employing sodium vapour, employ a mixture of various vapours. You see now that each ray given by these substances, instead of building up a line image, is building up for us a ring image—that we have now red, green, yellow, blue, and violet rings.

Corona itself used as a slit.

Now that was the consideration which led to the adoption of one of the new attempts to investigate the nature of the corona used this time. It was, to use a train of prisms, pure and simple, using the corona as the slit, a large number of prisms being necessary to separate the various rings we hope to see, by reason of their strong dispersion. On the screen the rings to a certain extent intersect each other; and it would have been easier to show you the ring-form of the images if we could have used more prisms than one.

If this is good for a train of prisms such as I have referred to, it is good for a single prism in front of the object-glass of a telescope. Such was the method adopted by Professor Respighi, the distinguished Director of the Observatory of the Capitol of Rome, who accompanied the expedition.

Now you may ask how would this method, if it succeeded, be superior to the ordinary one? In this way. If we were dealing merely with reflection, then all the rings formed by vapours of equal brilliancy at the base of the chromosphere would be of the same height, while if reflexion were not at work the rings would vary according to the actual height of the vapours in the sun's atmosphere, and the question would be still further advanced if the spectrum did not contain a ring representing the substance which underlies the hydrogen.

New equipment.

Our *new* spectroscopic equipment then was as follows:—

1. A train of five prisms.

2. A large prism of small angle placed before the object-glass of a telescope.

3. Integrating spectroscopes *driven by clockwork.*

4. A self-registering integrating spectroscope, furnished with telescopes and collimators of large aperture, and large prisms. (This instrument was lent by Lord Lindsay.)

Polariscopic instruments.

Now a word about the polariscopic instruments, referring you to my lecture given last year for a general notion of the basis of this class of observation.

A new idea was that observations to determine the polarization of the corona might be made with the same telescope and eye, both with the Biquartz and the Savart.

By the kindness of Mr. Spottiswoode, who has placed his magnificent polarizing apparatus at our service, I hope to be able to show you on the screen the mode of examining the corona by means of those two instruments, so as to enable you pretty well to follow what was actually done.

The Biquartz.

Let me begin with the Biquartz polariscope. In the first instance I will throw on the screen a representation of the corona itself, and we will then insert a Biquartz, and see its effect when I flood the screen with polarized light. You now see an indication of what would be observed supposing the polarization was due to polarized light diffused in the region between us and the dark moon and eclipsed sun; in which case the polariscopic effect would be observed generally over the dark moon, the corona and the region of the sky outside the corona. But this is not all, not only does this arrangement enable us to determine the existence of such a general polarization, but the vertical line in the Biquartz called the line of junction, when the colours on both sides of it are the same, indicates the plane of polarization ; so that we have two colours strongly contrasted in either half of the field when we are away from the plane of polarization, and a uniform colouring of the whole field when in or at right angles to that plane. By turning this prism through 90°, you see I entirely change the colours.

The Savart.

But we are not limited to the Biquartz in this inquiry. We can apply the Savart polariscope. Having still our image of the corona on the screen, I now replace the Biquartz by a Savart.

We now no longer see a line of junction with the similar or different colours on either side of it, but lines of colour running across the image. I turn the prism. We first see the lines with a white centre, then with a dark one : while at times they are altogether absent. And as a departure from the plane, when we use the Biquartz, gives us the strongest contrasts of colour, so you observe that with the Savart under these circumstances all indications of polarization vanish.

Now, if we assume polarization to be general, and the plane of polarization vertical, we should get those coloured bands, as you see them there, crossing the corona and dark moon, the lines being vertical and dark-centred. If the plane of polarization were horizontal, we should find the lines horizontal and the central one white.

Radial polarization.

But so far as we have gone, we have been dealing with polarization which is general, and we have not attempted to localize polarization at the corona itself. But I have here an apparatus, by means of which, quietly, in this theatre one can see as admirable an example as we should desire of polarization assumed to be particular to the sun and not general—I mean *radial polarization.* We have simply a circular piece of mahogany, or something else which polarizes light equally well, with a hole in the middle with sloping sides, cut as you see this cut, and then we place behind it a candle, so that the light of the candle, after falling on oiled tissue paper stretched across the aperture, can be reflected to the eye by the sides, the direct light of the candle being stopped by a central metallic diaphragm. We have now a source of polarized light of a different kind from the last. The next thing we have to do is to introduce into a small telescope exactly the same kind of apparatus we have there, though of course on a much smaller scale, and examine the ring of light seen when we put the candle behind the aperture. On examining the ring of light which is now visible by means of this telescope, which contains a Biquartz and analyzer, I see the most exquisite gradations of colour on either side the line of junction which cuts the field of view and the bright ring in the centre into two.

How observed.

Now, instead of the candle, we will employ the electric lamp; and instead of the eye, the screen; but I must inform you that the great heat of the electric lamp prevents the appearance being perfectly successful on the screen, as the reflecting varnish is melted.

In this experiment we cannot work with an image of the corona. We must make our corona out of the image of the ring we hope to get on the screen; and then, by employing the Biquartz in the same way as before; instead of getting similar colours on either side of the line of junction, as we did when we were working in the plane of polarization, and getting the greatest contrasts, as we did when we worked 45° away, you observe we get different colours in each part of the ring.

Experimental demonstration.

On the screen we now have a highly-magnified image of the hollow cone of iron which I am compelling to reflect the light from the lamp; and by inserting this Biquartz I throw various colours over different portions of that ring, which I beg you to consider for one moment as the solar corona, and the colours change as I rotate this prism. You will at once be able to explain the different actions of the Biquartz in this instance. The reflexion, and therefore the plane of polarization, is no longer general, but varies from point to point of the reflecting surface. It is in fact radial, and hence the delicate radiate arrangement of colour.

Such then were some of the new methods and new instruments we used for the first time in our researches. And I hope you will allow me to use this term, although our work was conducted a long way from the Royal Institution, the natural home of research in England.

The certain results.

I must now state very briefly some of the results of our work ; and first, the certain results.

Structure of corona.

We were able to make out the structure of the corona. We know all about the corona so far as the structure of its lower brighter strata, that portion, viz., which I referred to in my lecture last year[1] as being visible both before and after totality, is concerned. You may define it as consisting of *cool prominences* ; that is to say, if you examine a prominence any day, without waiting for an eclipse, and then go to an eclipse and examine the lower portion of the corona, you will find the same phenomena, minus the brightness. You find the delicate thread-like filaments which you are now all so familiar with in prominences—filaments which were first thrown on a screen in this theatre; the cloudy light masses; the mottling; the nebulous structure, are all absolutely produced in the corona, as far as I could see it with a telescope with an aperture of 6 inches ; and I may add that the portion some 5′ round the sun, reminded me forcibly, in parts, of the nebula of Orion, and of that surrounding η Argus, as depicted by Sir John Herschel, in his Cape observations.

We have shown that the idea that we did not get hydrogen above 10′ above the sun is erroneous; for we obtained evidence that hydrogen exists to a height of 8′ or 10′ at least above the sun; and I need not tell you the extreme importance of this determination. One of the proofs we have of that lies in this diagram, showing the observations made by Professor Respighi, armed with an instrument, the principle of which I hope you are now familiar with.

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[1] See p. 281.

Hydrogen higher than formerly thought.

Just after the sun disappeared Professor Respighi employed this prism to determine the materials of which the prominences which were then being eclipsed were composed; and he got the prominences shaped out in red, in yellow, in blue, and in violet light; a background of impure spectrum filling the field; and then, as the moon swept over the prominences, these images became invisible; he saw the impure spectrum and the yellow and violet rings gradually die out, and then three bright and broad *rings*, painted in red, green, and blue, gradually form in the field of view of his instrument; and as long as the more brilliant prominences were invisible on both sides of the sun, he saw these magnificent rings, which threw him in a state of ecstasy. And well they might.

Not glare.

These rings were formed by C and F, which shows us that hydrogen extends at least 7′ high, for *had we not been dealing with hydrogen we should have got a yellow ring as well*, because the substance which underlies the hydrogen is more brilliant than the hydrogen itself, and in addition to the red ring and the blue ring, which indicate the spectrum of hydrogen, he saw a bright green ring, much more brilliant than the others, built up by the unknown substance which gives us the Kirchhoff line, 1474.

Now at the time that Professor Respighi was observing these beautiful rings by means of a single prism and a telescope of some four inches aperture, some 300 miles away from him—he was at Poodocottah and I was at Bekul—I had arranged the train of prisms which you see here so that the light of the sun should enter the first prism, and after leaving the last one should enter my eye.

Comparison of Respighi's and my own observations.

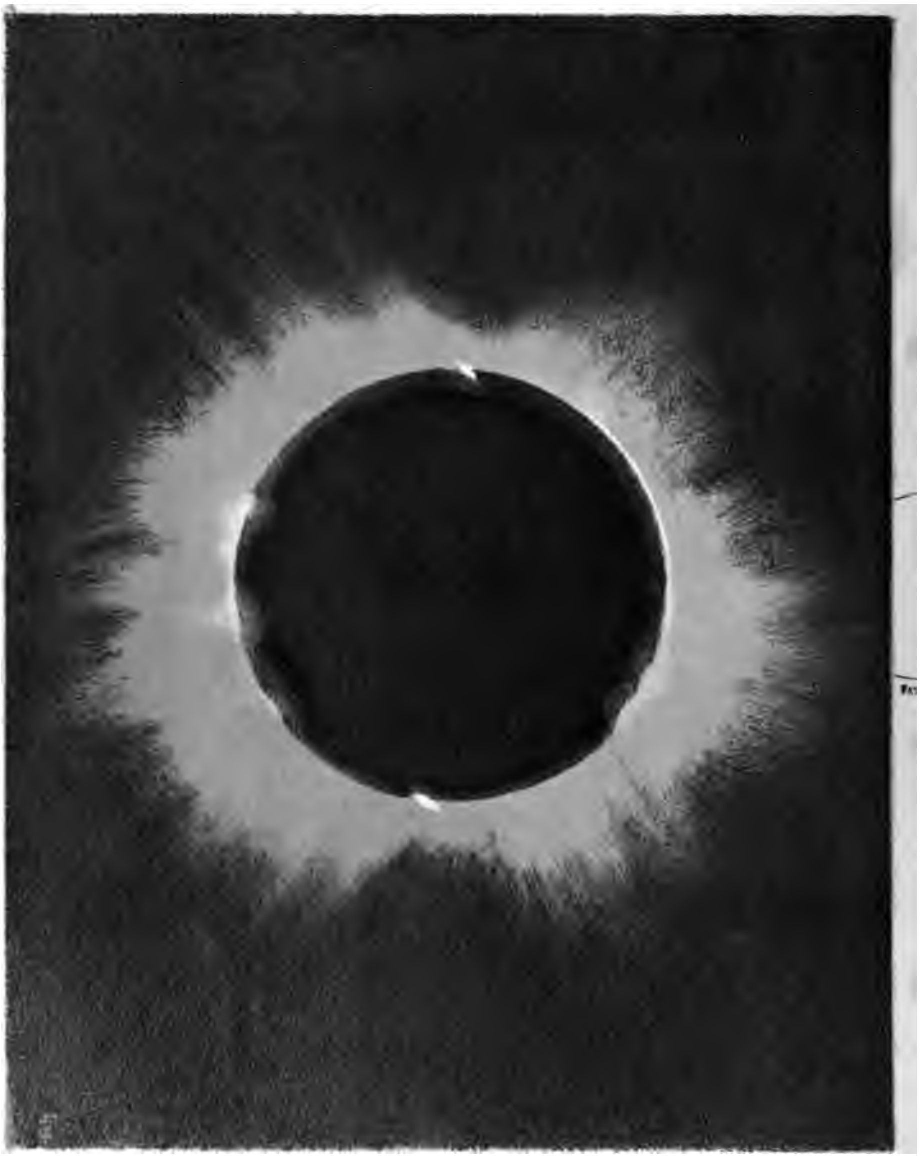
And what I saw is shown, side by side with Respighi's observations, in this diagram, in which I have separated the rings somewhat, so that there should be less confusion than in the actual observation. Here is Professor Respighi's first observation. He gets indications of C, D^3, F, and the hydrogen line near G. He was observing the very lowest, brightest region of all, and therefore 1474 was obliterated by the brightness of the continuous spectrum; but as the eclipse went on D^3 was entirely obliterated, and afterwards he got C and F building up rings together with 1474, which was not represented in the lower regions of the prominence—not because it was not there, but because, as I have already insisted, of the extreme brilliancy of the background. Now my observation was made intermediately, as it were, between the two observations of Professor Respighi's. Let me show the observations together.

Respighi .	C	D^3		F	G	Prominences at beginning of eclipse.
Lockyer .	C		1474	F	G	Corona at 80″ from commencement.
Respighi .	C		1474	F		Corona at mid eclipse.

Note that I had no object-glass to collect light, but that I had more prisms to disperse it; so that with me the rings were not so high as those observed by Respighi, because I had not so much light to work with: but such as they were I saw them better because the continuous spectrum was more dispersed, and because, with my dispersion, the rings—the images of the corona—therefore did not so much overlap. Hence, doubtless, Respighi missed the violet ring which I saw; so faint, however, that both that and 1474 were almost invisible, while C shot out with marvellous brilliancy, and D^3 was absent.

These observations thus tend to show, therefore, that instead of the element—the line of which corresponds with 1474—existing alone just above the prominences, the hydrogen accompanics it to what may be termed a great

Pl V. to face page 375.



THE PHOTOGRAPHIC CORONA 1871

height above the more intensely heated lower levels of the chromosphere, including the prominences in which the lower vapours are thrown a greater height. With a spectroscope of small dispersion attached to the largest mirror of smallest focus which I could obtain in England, the gaseous nature of the spectrum, as indicated by its *structure*, that is, bands of light and darker intervals as distinguished from a continuous spectrum properly so called, was also rendered evident.

Gaseous spectrum.

These are results of the highest importance, which alone are worth all the anxiety and labour connected with the expedition.

Photographic results.

But there is more behind. The photographic operations (part of the expense of which was borne by Lord Lindsay) were most satisfactory, and the solar corona was photographed to a greater height than it was observed by the spectroscope, and with details which were not observed in the spectroscope.

Mr. Davis was fortunate enough to take an admirable series of five photographs at Bekul, and Captain Hogg also obtained some at Jaffna; but I am sorry to say the latter lack somewhat in detail.

I have prepared two lamps, because I am anxious to exhibit the photographs two at a time, that you may compare one with the other. [This was done.] You see that so far as the camera goes—and mark this well—the corona was almost changeless during the whole period of totality; this is true, not only for one place, but for all the places at which it was photographed.

I now exhibit two other photographs—one taken at Jaffna and the other at Ootacamund. Actinically the corona was the same and practically changeless at all the stations. You see that, though not so obvious as in the other case, there is the same similarity.

Before I leave the actinic corona, I am anxious to show you an image of it, taken during the American eclipse of 1869 in a camera exposed to the sun during the whole of the totality; to a certain extent in our recent photographs we have reproduced what was photographed in 1869.

The solar nature of most, if not all, of the corona recorded on the plates is established by the fact that the plates, taken in different places, and both at the beginning and end of totality, closely resemble each other, and much of the exterior detailed structure is a continuation of that observed in the inner portion independently determined by the spectroscope to belong to the sun.

While both in the prism and the 6 inch equatorial the corona seemed to form pretty regular rings round the dark moon, of different heights according to the amount of light utilized by the instrument; on the photographic plates the corona, which, as I have before stated, exceeds the limits actually seen in the instrument I have named, has a very irregular, somewhat stellate outline; most marked breaks or rifts (*ignored by the spectroscope*), occurring near the sun's poles, a fact perhaps connected with the other fact that the most active and most brilliant prominences rarely occur there.

The drawings.

From the photographs in which the corona is depicted actinically we pass to the drawings in which it is depicted visually. I would first call attention to two drawings made by Mr. Holiday, who formed part of the expedition, and in whose eye every one who knows him will have every confidence.

First there is a drawing made at the commencement of the totality, and then a drawing made at the end. There is a wonderful difference between the drawings; the corona is in them very much more extensive than is represented actinically on our plates.

Here is another drawing, made by Captain Tupman, in which, again, we have something absolutely different from the photographs and from Mr. Holiday's sketches, inasmuch as we get an infinite number radial of dark lines extending down to the moon, and a greater extension than in the photographs, though in places the shape of the actinic corona and some of its details are shown.

Naked eye corona.

Now the corona, as it appeared to me with the naked eye, was nothing but an assemblage of bright and dark lines, it lacked all the structure of the photographs and appeared larger; and I have asked myself whether these lines do not in some way depend on the size of the telescope, or the absence of a telescope. It seems as if observations of the corona with the naked eye, or with a telescope of small power, may give us such lines; but that when we use a telescope of large power, it will give, close to the moon, the structure to which I have referred, and abolish the exterior structure altogether, leaving a ring round the dark body of the moon, such as Professor Respighi and myself saw in our prisms and in the 6-inch telescope, in which the light was reduced by high magnification so as to bring the corona to a definite ring some 5′ high, while Professor Respighi, using a 4-inch telescope and less magnifying power, brought the corona to a ring something like 7′ high.

Possible origin of ring-formed coronas.

And here we have an important connection between spectroscopic and telescopic work. If we employ a telescope in which the light is small or is reduced by high magnification, we bring the corona to a definite ring, and perhaps here we have the origin of the 'ring-formed' coronas.

Many instances of changing rays, like those seen by Plantamour in 1860, were recorded by observers in whom I have every confidence. One observer noted that the rays revolved and disappeared over the rifts.

Polariscopic observations.

We have next to deal with the polariscopic observations.

Mr. Lewis, in sweeping round the corona at a distance of 6′ or 7′ from the sun's limb, using a pair of compensating quartz wedges as an analyzer, which remained parallel to itself while the telescope swept round, observed the bands gradually change in intensity, then disappear, bands of a complementary character afterwards appearing, thereby indicating radial polarization.

Dr. Thomson at Bekul saw strong traces of atmospheric, but none of radial, polarization, with a Savart. With the same class of instrument the result obtained by myself was precisely similar; while on turning in the Biquartz, at the top and bottom of the image of the corona, *i.e.* near the sun's equator, faint traces of radial polarization were perceptible for a short distance from the moon's limb. Captain Tupman, who observed with the polariscope after totality, announces strong radial polarization extending to a very considerable distance from the dark moon.

Base of corona.

Leaving the extreme outside of the corona as a question to be determined at some future time—and it can well wait—let us come to the base of the corona, and deal with the region to which I have already referred, close to the sun.

What was the general conclusion at which we arrived on this important point? Before I state it, let me tell you the instrumental conditions of the inquiry. We can use such a spectroscope as the one with which you are all familiar, and so arrange matters that the slit shall be carried by a clock, so that it may follow accurately the edge of the moon; but if the least variation in the rate of motion takes place, the observation is rendered almost valueless. But if we employ a spectroscope, in which we sum up the light—do not localize the light, but throw it together—it does not matter whether your clock goes well or not, you are certain to have a result worthy of credit. But if you employ such an instrument as Professor Respighi employed, and abolish the slit altogether, the weight of any observations made with such conditions is very great.

Captain Maclear, who was observing with me at Bekul, has undoubtedly shown that when the light of our atmosphere is cut off by the interposition of the dark moon, we see very many more bright lines than we do when this is not the case, the lines being of unequal height.

Mr. Pringle, also at Bekul, showed that, at the end of totality many lines flashed into one of these instruments, carried under these difficult conditions.

Captain Fyers, the Surveyor-General of Ceylon, observing with a spectroscope of the second kind, saw something like a reversal of all the lines at the beginning, but nothing of the kind at the end.

Mr. Fergusson, observing with a similar instrument, saw reversal neither at the beginning nor the end.

Mr. Mosely, whose observations are of great weight, says that at the beginning of the eclipse he did not see this reversal of lines. Whether it was visible at the end he could not tell, because at the close the slit had travelled off the edge of the moon.

Professor Respighi, using no slit whatever, and being under the best conditions for seeing the reversal of the lines, certainly did not see it at the beginning, but he considers he saw it at the end, though about this he is doubtful.

Conclusion.

From the foregoing general statement of the observations made on the eclipse of last year, it will be seen that knowledge has been very greatly advanced, and that most important data have been obtained to aid in the discussion of former observations. Further, many of the questions raised by the recent observations make it imperatively necessary that future eclipses should be carefully observed, as periodic changes in the corona may then possibly be found to occur. In these observations the instruments above described should be considered normal, and they should be added to as much as possible.

I had intended, if time had permitted me, to point out how much better we are prepared for the observation of an eclipse now than we were when we went to India, and how a system of photograph record should be introduced into the spectroscopic and polariscopic work; but time will not allow me to do more than suggest this interesting topic. I am anxious, however, that you should allow me one minute more to say how very grateful we feel for the assistance rendered by all we met, to which assistance so much of our success must be ascribed. I wish thus publicly to express the extreme gratitude of every one of our expedition to the authorities in India and Ceylon for the assistance we received from them; and our sorrow that Admiral Cockburn, a warm and well-known friend to Science, who placed his flagship at the disposal of the expedition, and the Viceroy, whose influence in our favour was felt in every region of India whither our parties went, and to whom we gave up our ship, are now, alas! beyond the expression of our thanks. We are also anxious to express our obligations to the directors and officers of the Peninsular and Oriental Company for the magnificent way in which they aided us. If they had not assisted us as they did, Science would have gained very much less than she has done from the observations of the last eclipse.

THREE YEARS' WORK WITH THE NEW METHOD.[1]

CHAP. XXV.

IN the two lectures I delivered in this Theatre three years ago I endeavoured to bring before you some of the first results which had been obtained by employing a new method in solar research—that method, namely, which enables us to study now one little bit and now another of the sun. And in order to place these results as clearly as possible before you, I sketched the work which had been done since the invention of the telescope, remarking that since that time not only has the power of our telescopes increased, but we have had first one instrument of research and then another added, so to speak, to our scientific stock-in-trade. As I am now to place before you some of the results which have crowned the efforts which have been made to increase our knowledge of the sun during the last three years, I shall endeavour to connect the new work with the old this time, pretty much as I endeavoured to do upon the last occasion. Nor is this all. Such great advances have been made that it will be necessary for me to split up the subject, in order that the different lines of research may be the better understood, and that my own work will be the easier, for I quite feel the difficulties connected with the giving of such a general *résumé* of entirely new work over so large a field.

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[1] Revised from the short-hand notes of two lectures delivered before the Literary and Philosophical Society of Newcastle-upon-Tyne, in October 1872.

Looking at the new results, then, from the spectrum point of view—that is, arranging them as the spectrum is affected by the various solar phenomena with which the new method has familiarized us, we may group it as follows :—

Changes in the spectrum.

1. Variations in the positions of lines in the solar spectrum (changes of wave-length), connected with changes in the lines from dark to bright.

2. Variations in the thickness of spectral lines (selective radiation and absorption), and in the amount of continuous absorption.

A moment's thought, however, will convince you that this grouping is too general; and that it will be better to deal with the solar phenomena themselves, and to state under each head the advances made. This I shall do, but it will be convenient to say at the outset one word specially on the question of the changes of wave-length, as that comes in in every part of the inquiry.

CHANGES OF WAVE-LENGTH.

In my former lectures I stated the *rationale* of this new method of determining the velocities of the vapours in the solar atmosphere, and felt so certain about it that it never entered my mind that it could be questioned : it has been questioned, however, and it is my duty to record the fact.

Respighi.

Professor Respighi, an observer of great eminence, whose name you will often hear in the sequel, is inclined to attribute the phenomena observed to the disturbances in our air, or to the action of the heat of the sun's image on the slit, but he here forgets that such a cause would affect *all* the lines in the spectrum, and not a few isolated ones only. In a later memoir the illustrious Italian admits having seen the phenomena, but does not say whether he yet agrees with the explanation. Father Secchi also for a long time discredited it; while admitting that he had seen the phenomenon, he considered that too much importance had been ascribed to it, as he said he observed it to take place all round the sun's limb, and that a little reflection had convinced him that it was simply due to the effect of *the sun's rotation*. He stated that he had observed displacements of $\frac{1}{10}$ of the distance from D^1 to D^2, and that such a movement was equal to over 300 kilometres per second. He then proceeded to calculate the rate of the solar equatorial rotation, and made it 429 kilometres per second—a mistake of a most remarkable kind, as it is over 200 times the true rate of that motion. This error was immediately indicated by Fizeau, whereupon Secchi hastened to correct it, at the same time stating that the true rate of 1.92 kilometres did not in any way invalidate his explanation, but, on the contrary, agreed better with the *smallness of displacement* observed by him, a displacement which be it remembered he at first stated could not be produced by a rate less than 300 kilometres per second. But I am glad to say that Father Secchi has now roundly stated his conviction of the accuracy of my announcement. M. Rayet, also, in a memoir otherwise admirable from several points of view, has referred to my observations on the changes in question as "les illusions de M. Lockyer;" but Professor Young from the first has observed and recorded phenomena exactly similar to those I described to you, and has from the first accepted my explanation of them. Still later, another astronomer, Dr. H. C. Vogel, of Bothkamp, has supported my views. So that now I think I am not saying too much in stating that this matter is quite established.

Secchi.

Rayet.

Young.

Vogel.

I shall refer to the changes of the lines from dark to bright further on.

SPOTS GENERALLY.

I. Connection with Planetary Configurations.

We will begin our more detailed consideration of the recent work with a statement of the progress made in our knowledge of sun-spots. These spots are already familiar to you; and you may also recollect that I referred to the magnificent researches of Carrington and of the Kew observers, which have unfolded to us many facts connected with the origin and nature of them. I also told you that a discussion of the appearances presented by the spots had given rise to two distinct theories of their origin, and that a test of the most conclusive nature had been applied by the new method of research.

Spot phenomena occur at a certain level.

It was next pointed out that the Spot phenomena were obviously located in a particular *niveau* of the sun's atmosphere, and that probably there might be conditions under which this particular level might vary from time to time. I also showed you that by carefully noting the lines, which are most affected by selective absorption, we could determine the particular vapours which were at work in doing the absorption for us, so far as the selective portion of it was concerned. And obviously, if we had any change in the absorption going on in a spot—that is to say, if in one spot we get iron doing the absorbing work, and in another spot sodium, we should at once be able to separate the iron-spot, so to speak, from the sodium-spot. And further, if we could locate in the sun's atmosphere these particular vapours which cause absorption, by supposing, for instance, the sodium vapour to be higher up in the solar atmosphere than the iron vapour, then we should be justified in assuming that a spot in which we saw a great selective absorption due to the vapour of sodium was situated at a higher level in the solar atmosphere than one which showed very strong indications of iron absorption;

Sodium and iron spots.

and similarly, if at one time we get iron perpetually brought before us in the spots, and at another time we get sodium, then, in those cases, we should get a difference due to time, as in the other case we got a difference due to place. Further, I pointed out to you that from the hydrogen to the iron there was a running down in the rate of motion of the vapours, and that, associated with these indications of general and of varying selective absorption, we had very often a perfectly distinct phenomenon—that is, the sudden brightening of many of the lines in the spectrum: the lines of hydrogen, sodium, and magnesium, for instance.

Secular and local variations observed.

In what I said about the sun-spots I was careful to refer to the work of Carrington and Balfour Stewart, De La Rue and Loewy, work which has largely increased our knowledge of the distribution and motion of the spots, so that our knowledge of the sun-spot curve—one of about eleven years period, during which we have no spots on the sun, a few spots, and then a maximum of spots; coming back after eleven years to the period of no spots, or of minimum sun-spots again—is rapidly getting more exact. Our knowledge of the true shape of this curve has, for instance, been increased by some researches of Wolf and Fritz,[1] from which it seems that instead of running over a period of eleven years and 1-10th, the period is only of eleven years and 7-100ths. That, you will say, is a very small matter. Another small matter, but one which in the long run will probably prove to be of considerable importance, is, that the descending part of the curve is always associated, and varies with the previous ascending portion of the curve. Moreover, the curve requires three years and a half to ascend and seven years and a half to descend.[2] So that when we have a period of maximum sun-spots, such as we had last year, we may expect the spots and associated phenomena to be much longer in running down than they were in attaining their maximum.

The sun-spot curve.

Relations of various parts of the curve to each other.

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[1] *Proc. R. S.* vol. xix. p. 392.

[2] De La Rue, Stewart, and Loewy, *Proc. R. S.* vol. xx. p. 82.

I must also refer to a very important confirmation of the idea that sun-spots are in some way connected with planetary configurations, and it looks very much as if—I say it looks "as if," because probably many of you will not consider the evidence as yet conclusive,—most of the phenomena depend upon some influence exerted by Venus and Mercury. In a paper which has been communicated to the Royal Society during the present year, the Kew observers show that the average size of a spot would appear to attain its maximum on that side of the sun which is turned away from Venus or from Mercury, and to have its minimum in the neighbourhood of Venus or of Mercury. In other words, dealing with the part of the sun-spot action which is due to Venus, if you lived on the planet Venus you would never see a sun-spot; and dealing with that portion which refers to the planet Mercury, if you lived on the planet Mercury you would never see a sun-spot—so that as the maximum is to be found on the side of the sun which is turned away from Venus or from Mercury, naturally a minimum is seen in the neighbourhood of Venus or Mercury, as seen from the earth; and the appearance and behaviour of this kind is so decided with regard to Venus that the Kew observers have been able to take the observations *en masse*—that is to say, all the observations taken by Carrington, and all the observations taken by themselves; and they have been able to divide them into the Carrington observations and the Kew observations, and they lead absolutely to the same result, namely, that Venus and Mercury have something to do with the birth and death of the spots on the sun.

Probable influence of Venus and Mercury.

II. Sun-spot Spectra.

Let us next pass to spectroscopic work, and see how our knowledge has been advanced in the matter of sun-spot spectra. Is our knowledge of these phenomena much greater now than when I last addressed you? Yes, it is. But I should add that, as was to be expected, a great many of the explanations of the new phenomena observed, which have been given, not only in this part of the inquiry but in others, as you will see as we go on, have not been universally accepted. For instance, instead of referring the phenomenon presented by sun-spots to the continuous absorption and to the greater density of particular vapours in the sun, Father Secchi has announced that the change from the ordinary spectrum is due to the presence of aqueous vapour in the sun-spots (see Note A). This conclusion I have not been able to endorse. And it has also been stated by another Italian observer, Professor Respighi, that the various selective absorption phenomena presented by sun-spots may depend upon our instruments, and is not real.

Secchi's view.

Respighi's view.

Passing from questions which have been discussed, we have a great deal of new work to place on record.

You already know that the thickness of a spectral line is an indication of the pressure of the gas or vapour which is radiating or absorbing the light. For instance, if we consider the lines D in the ordinary spectrum of any part of the sun, in a spot we find these lines considerably thickened; and if we get the vapour of sodium not *absorbing*, as it does in the case of a spot, but *radiating* as it does in a prominence, we neither have the line of medium thickness, as you see it in the photosphere, nor of considerable thickness, as you see it in a spot, but of an almost inconceivable thinness. Now then, the spectroscope has afforded us evidence that the sun-spots are certainly not what they were three years ago. The spectral lines over the umbra and penumbra are very much thinner; in other words, the indications of absorption, both continuous and selective, are very much less. Of course, in the telescope the record would run that the umbra and penumbra are much darker at some times, probably near the sun-spot minimum, than at others; and in a conversation which I had with Mr. Howlett, one of our most indefatigable sun-spot observers, on the question, some time ago, he stated it as his

Thinness or thickness of lines = high or low pressure.

The absorption is less than it was three years ago.

impression that this is so. If a longer series of observations endorses this, we shall have evidence that at one period the spots occur under conditions of greater pressure than at others.

New research added.

Hence we have added a new branch of research in our studies of the sun-spot cycle, quite independent of the mere number of spots upon the sun, and it is probably the best means that we have for adding new elements to those which we already know to exist in the sun's atmosphere, and of determining a great many problems of the highest interest, not only to the astronomer but to the chemist. But we have done very much more than this. In this class of observations we have three main points to be attended to: first the lines which are thickened, as first observed by me in 1866; next those which are brightened by prominences floating over spots[1] (April 1869), and lastly the indications of motion before referred to.

Three main points of the inquiry.

Now with regard to the first point: to sodium, magnesium, &c. before mentioned, Father Secchi soon added calcium and other substances, and Professor Young has enlarged the list by the addition of titanium.[2]

In the case of the bright lines observed over spots, the bright lines are so much thinner than the dark ones, that the dark one may be considered as a sort of window, and in the breadth of the window, details of the prominence cloud can be seen. This phenomenon I had the pleasure of showing to Professor Roscoe on May 12th, 1869, having already got the hydrogen, sodium, and magnesium lines thus reversed. In the reversal of the hydrogen lines, I was soon endorsed by Father Secchi and Professor Young, both of whom later saw the other lines reversed also. In some observations with which Professor Young has enriched science on this point, he has given us some of the most beautiful detailed spectroscopic work with

Secchi and Young endorse the reversal of the hydrogen lines.

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[1] I include these phenomena under the heading of sun-spots, not because they are limited to sun-spot regions, but because they are generally observed there.

[2] *Journal of the Franklin Institute*, July 1870.

which I am acquainted. Allow me to quote from these observations.[1]

On September 22nd, 1870, Professor Young saw the sodium lines D_1 and D_2 reversed in the spectrum of the umbra of a large spot near the eastern limb of the sun. At the same time the C and F lines were also reversed.

Reversal of sodium lines over a spot.

The figure gives the appearance of the sodium lines. In the umbra of the spot the D_3 line was not visible, but in the penumbra was plainly seen, as a dark shade, represented in the figure.

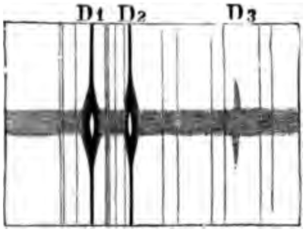

FIG. 122 —Young's observation of the reversal of the sodium lines over a spot.

"In the spot-spectrum the magnesium lines b_1, b_2, and b_4 were not reversed, but while the shade which accompanies the lines was perceptibly widened, the central black line itself was thinned and lightened."

Nickel in motion and magnesium at rest.

In another communication he gives a large list of lines which he observed in several of the spots which then appeared upon the face of the sun. He states that once when the magnesium line *b* showed that vapour to be in a state of perfect rest, the included nickel line indicated enormous motion by the changes of wave-length which it underwent, from a cause which has already been completely explained to you.[2] He then also states that, in the case of some of the spots he has succeeded with absolute distinctness in locating in the prominence near the spot the regions to which the various vapours were confined. Thus the line in the yellow part of the spectrum, which some interested in these matters still consider to be a line due to hydrogen, was on one occasion confined to the central part of a prominence over a sun-spot, while he saw the hydrogen lines extending far beyond this region. This is a piece of important evidence on a point which has been touched upon in laboratory experiments, and in the last Indian eclipse, to which I shall again refer. I may here add that we must now associate, I think, these bright prominences, built up of vapours giving us various spectral lines, with the colours which now for many years past have been telescopically observed on the umbræ of spots in Italy, America, and England; and great credit is due to Father Secchi for giving great attention to this point in his recently published work.

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[1] *Journal of the Franklin Institute*, 1870, p. 232*a*.

[2] Communication to the *Journal of the Franklin Institute*, dated October 3rd, 1870.

Colours of the spots.

I will now pass on from the consideration of the spots, and in doing so, I must ask you to bear with me if my story seems too detailed, for as any branch of science advances, the free bold strokes by which one may very easily give a sketch of some of the main features, gradually have to give way to the detailed filling-in.

PROMINENCES.

I. General Remarks.

We come next to the question of solar prominences. On the former occasion I endeavoured to tell you the story of the prominences, referring to the observations down to the American eclipse of 1869; and stated that, by means of the observations made during the Indian eclipse of 1868, the American eclipse of 1869, and by the new method, the materials of these prominences had been to a certain extent determined, that evidences of very great changes in the prominences had been detected, that a method of observing the rate of movement of the prominences, both their wind-like movement along the sun's atmosphere, and their up-and-down move-

Materials of the prominences determined.

ment in the sun's atmosphere, had been discovered; and I also showed you that in addition to that, the new method gave us indication of a general welling-up of the lower vapours—magnesium and iron, for instance—from time to time. And I also lastly pointed out that the prominences had been associated with a stratum which was continuous round the sun. Now, although these conclusions have not all been readily accepted, I think we now may say that the facts which I pointed out to you in 1870 have all of them been established.

Young photographs a prominence.

Prof. Young has succeeded in obtaining photographs of a protuberance, of which he has been good enough to forward me a specimen. It was obtained by attaching a small camera to the eye-piece of the telescope and opening the slit somewhat widely, using the hydrogen line near G. He adds:—"As a picture, the little thing amounts to nothing, because the unsteadiness of the air and the maladjustment of the polar axis of the equatorial caused the image to shift its place slightly during the long exposure of three-and-a-half minutes which was required, thus destroying all the details. Still, the double-headed form of the prominence is evident, and the possibility of taking such photographs is established."[1]

At the present moment it is impossible to predict what the ultimate form of such researches will be, but it is clear that before long we shall have a photographic daily record of all solar phenomena.

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[1] The following is Professor Young's account of his work, as communicated to the *Journal of the Franklin Institute*, Oct. 3, 1870.

"The protuberances are so well seen through the F and 2796 (*near* G) lines, that it is even possible to photograph them, though perhaps not satisfactorily with so small a telescope as the one at my command. Some experiments I have recently made show that the time of exposure, with ordinary portrait collodion, must be nearly four minutes, in order to produce images of a size which would correspond to a picture of the solar disc about two inches in diameter. This length of exposure demands a more perfect clockwork than my instrument possesses, and a more accurate adjustment of the polar axis than it had during the experiments, as well as a steadier condition of the atmosphere.

"Thus far, therefore, I have not been able to produce anything which could properly be called a good picture. Negatives have been made which show clearly the presence and general form of protuberances, but the definition of details is unsatisfactory. This amount of success was reached upon September 28th, when impressions were obtained of two protuberances on the S.E. limb of the sun, and, slight as this success was in itself, I consider it of importance in showing the perfect feasibility of going much further with more sensitive chemicals, more delicate adjustments, and greater telescopic power. I was aided in the experiments by Mr. H. O. Bly, our local photographer, to whom are due my warmest acknowledgments for the interest, patience, ingenuity and skill, with which he assisted me.

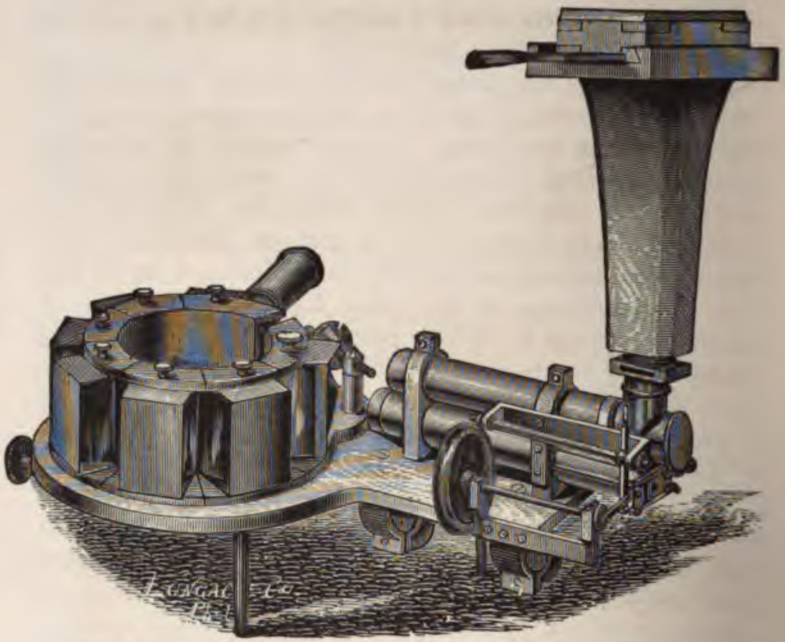

FIG. 123.—Spectroscope, with camera attached.

"We worked through the hydrogen γ line (2796 of Kirchhoff's scale) which, though very faint to the eye, was found to be decidedly superior to F in actinic power. The photographic apparatus employed consisted merely of a wooden tube, about 6 inches long, attached at one end to the eye-piece of the spectroscope, and at the other carrying a light frame. In this frame was placed a small plate-holder, containing for a sensitive-plate an ordinary microscope slide, 3 inches by 1.

II. Forms and Classes of Prominences.

There is one thing in which we have made progress, this is, that all the workers are now, I think, quite content to divide the prominences mainly into two classes, namely, eruptive and nebulous. This conclusion, which I

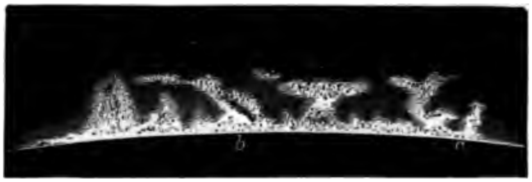

FIG. 124.—Cirrus mass of Prominences observed by Young, Sept. 4, 1869. Points at *a* and *b* very bright.[1]

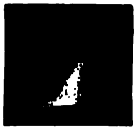

FIG. 125.—Small prominence.

FIG. 126.—A small forked prom inence.

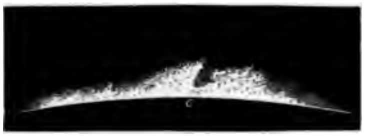

FIG. 127.—Cumulus Prominences like the fish mouth in the nebula of Orion. (Young, 4th Sept. 1869.)

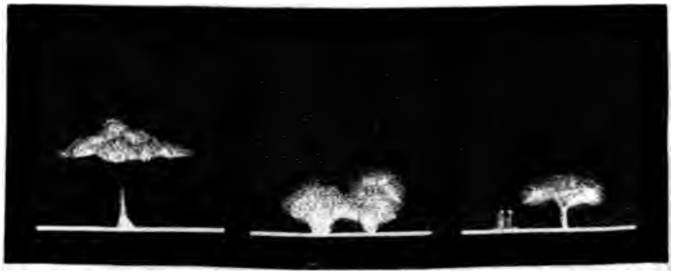

FIG. 128.—Forms of Prominences. (Zöllner.)

Two classes of prominences.

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[1] These drawings are taken from the *Journal of the Franklin Institute*, October 1869, p. 287.

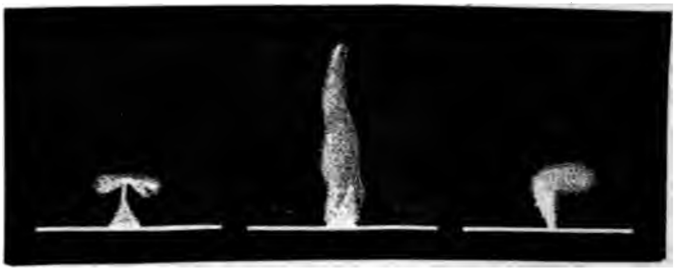

FIG. 129.—Forms of Prominences. (Zöllner.)

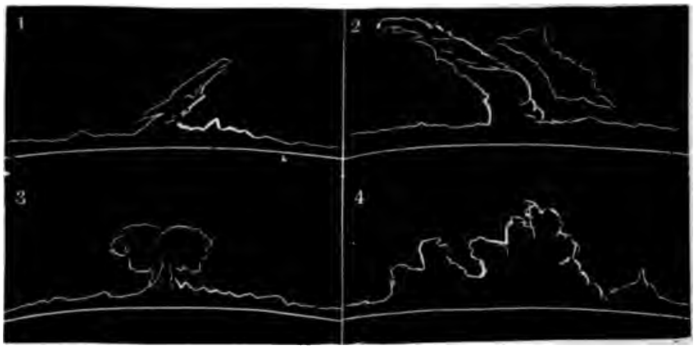

FIG. 130.—Forms of Prominences. (Young.)
1. Sept. 28, 1869; 2. Sept. 28, 1869; 3. Oct. 1, 1869; 4. Oct. 1, 1869.

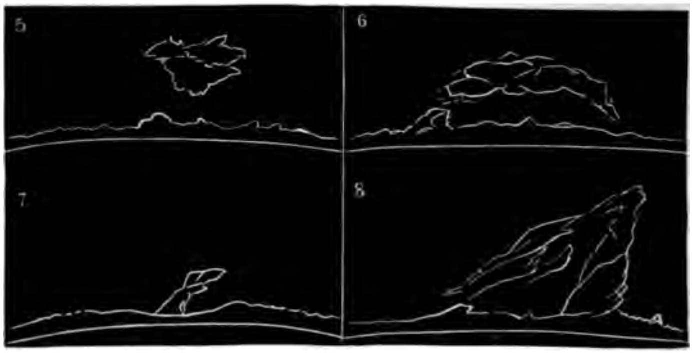

FIG. 131.—Forms of Prominences (Young.)
5. Oct. 2, 1869; 6. Oct. 11, 1869; 7. Nov. 4, 1869; 8. Nov. 4, 1869.

brought before you in the last lecture, has been accepted by Secchi, Zöllner, Spörer, and by Young,[1] and also by Professor Respighi, who, however, from his enormous study of the prominences—he has carefully mapped and drawn not less than 7,000—is inclined to sub-divide the eruptive class into a great many sub-classes.

Respighi's sub-classes.

III. Dimensions of the Protuberances.

I have just referred to the enormous number of prominences catalogued by Professor Respighi: I now propose to refer to his observations on their dimensions.

After remarking that—

Measurements do not always give the true height.

> "Our measurements do not generally give the true height of the protuberances, because often their lower part is more or less concealed by the solar disc, and therefore it is only by those protuberances, the bases of which are to be seen on the circumference or near to it, that the real height can be obtained."

He adds:—

> "In a long and continuous series of observations, since necessarily many protuberances are presented in the condition alluded to, so from the greatest heights observed the extreme limit may be considered as determined satisfactorily—that is, the height to which these masses of hydrogen may rise on the solar surface."

His evidence then runs as follows:—

Extreme height.

> "In the whole course of my observations I have never found a protuberance sensibly higher than 6′, and very few of them approach that limit.
>
> "The protuberances or jets which attain this extreme, and in general all great heights, are those produced by temporary or evanescent eruptions, which are developed at intervals in the locality of the spots, while in other protuberances, whether isolated or combined in groups, very seldom is the height above three minutes—about ten terrestrial diameters. A moderate number of them attain the height of 2′, very many more 1′, or more than three terrestrial diameters.
>
> "Without making a detailed statement of the protuberances ranged according to their apparent heights, in order to furnish some idea of

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[1] "About forty different prominences have been more or less carefully observed (from September 10 to October 3, 1870), sixteen have been sketched. Most of them fall naturally enough into the categories established by Zöllner and Lockyer."—*Journal of the Franklin Institute*, 1870.

the various activities of the solar eruptions, I will confine myself to giving the numbers of the protuberances which during the course of the observations have attained or exceeded the height of 1′, of 2′, of 3′, of 4′, and of 5′; from which can be inferred the degree of probability to be assigned to the production of phenomena within the indicated limits. Taking into account only the protuberances observed in the general figure of the solar circumference in 283 profiles, and leaving out the small jets and protuberances observed and drawn in the partial figures, and in the uncertain or incomplete profiles, the total number of the protuberances is 7,449. I must observe, however, that this number is obtained by dividing the compound protuberances or the large groups into the chief component groups, and the protuberances composed of fine jets, into the chief clusters of which they consist.

"Besides this total sum we have 1,363 protuberances which have attained or exceeded the height of 1′, which number may be divided as follows, according to their height :—

From	1′	to	2′,	number of the protuberances . . .	1,154
,,	2′	,,	3′	,, ,, ,, . . .	174
,,	3′	,,	4′	,, ,, ,, . . .	27
,,	4′	,,	5′	,, ,, ,, . . .	3
,,	5′	,,	6′	,, ,, ,, . . .	5

Per centage of prominences of different heights.

From these numbers, it is shown that of above 100 protuberances, about 18 attain and exceed the height of 1′; of above 1,000, about 28 attain or exceed the height of 2′; and of more than 10,000, about 47 attain or exceed the height of 3′. We must therefore consider protuberances which exceed the great height of 4′ as something quite extraordinary or exceptional."

IV. Change of Form.

With regard to the enormous change of form in the prominences, to which I before referred, Professor Young has given a description even more startling than the one which I ventured to bring before you. You may recollect that I showed you the picture of a prominence some 40,000 miles high, which had been shattered to atoms in some ten minutes.

Professor Young thus gives an account of some changes observed during the autumn of 1870 :—

Young's observations.

"Fig. 132 represents a small one which was observed upon the E. limb of the sun, on September 14th, about 4.30 P.M. From the point marked A, which was very brilliant, a small fragment detached itself and rose towards A′, enlarging in size and growing fainter as it rose. It disappeared (from faintness) in about 12½ minutes, at a distance of 2′ 30″ above the limb of the sun, as determined by the time, 8·5, which was occupied by the intervening space in passing over the slit of the spectroscope. Allowing for the obliquity of the motion to the parallel of declination, the length of path passed over by this cloud was more than 90,000 miles, and the velocity above 120 miles per second.

FIG. 132.—Rising of a detached fragment.

"Fig. 133 represents a prominence observed September 20th, at 4 P.M., on the S.E. limb. (Pos. S., 60° E.) It was a nearly vertical stream, made up of spindle-formed filaments, and had attained the enormous height of 3′ 20″ or 90,000 miles (determined, as in the case above mentioned, by a time-observation, corrected for inclination). It was very brilliant near the base, and at two or three other points along its length. At 4.30 it was nearly gone, only a few faint wisps of cloud remaining.

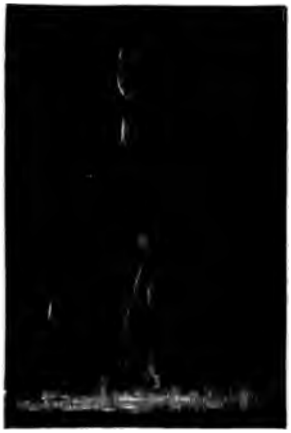

FIG. 133.—Filamentous prominence.

"Another, observed on September 27, at 4.10 P.M., and situated on the W. limb of the sun, is represented in Fig. 134. It was formed of separate, well-defined, narrow streamers, which appeared to consist of matter, first violently ejected, and then as violently deflected, by some force acting nearly at right angles. The altitude of the highest point was 1′ 25″, the length of the whole about 3′ 30″. I am unable to see how any mere projection from the sun could have produced such a form, and cannot help feeling that it indicates a *something* in which powerful currents may exist, even at such great elevations above the solar surface; in short, an atmosphere extending far beyond the limits which calculation would seem to assign as possible. Is it wholly unlikely that at such an enormous temperature the law of Mariotte may fail so completely as to destroy the reliability of any computation that assumes it as one of the data?

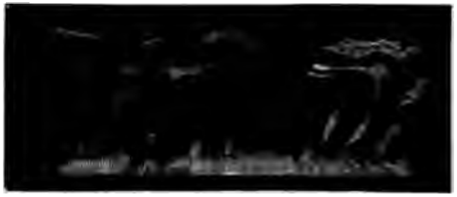

FIG. 134.—Streamer prominence.

Change of type.

"Upon the next day the prominence still persisted, but its type was wholly changed: it was replaced by one of the mushroom-formed masses which are so common."[1]

Professor Young has since told the story of another tremendous change of form. His description[2] is so vivid that I am sure you will wish me to quote it *in extenso.*

Another case.

"On the 7th of September, between half-past twelve and two P.M. there occurred an outburst of solar energy remarkable for its suddenness and violence. Just at noon the writer had been examining with the telespectroscope an enormous protuberance or hydrogen cloud on the eastern limb of the sun.

"It had remained with very little change since the preceding noon—a long, low, quiet-looking cloud, not very dense or brilliant, nor in any way remarkable except for its size. It was made up mostly of filaments nearly horizontal, and floated above the chromosphere with its lower surface at a height of some 15,000 miles, but was connected to it, as is usually the case, by three or four vertical columns brighter and more active than the rest. Lockyer compares such masses to a banyan grove. In length it measures 3′ 45″, and in elevation about 2′ to its upper surface—that is, since at the sun's distance 1″ equals 450 miles nearly, it was about 100,000 miles long by 54,000 high.

"At 12.30, when I was called away for a few minutes, there was no indication of what was about to happen, except that one of the connecting stems at the southern extremity of the cloud had grown considerably brighter, and was curiously bent to one side; and near the base of another at the northern end a little brilliant lump had developed itself, shaped much like a summer thunder-head. Fig. 135 represents the prominence at this time, *a* being the little 'thunder-head.'[1]

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[1] *Journal of the Franklin Institute*, November 1870.

[2] *Boston Journal of Chemistry*, November 1871, vol. vi. p. 49.

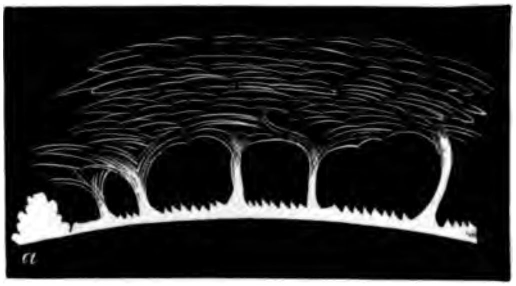

Fig. 135.—Tree-like prominences.

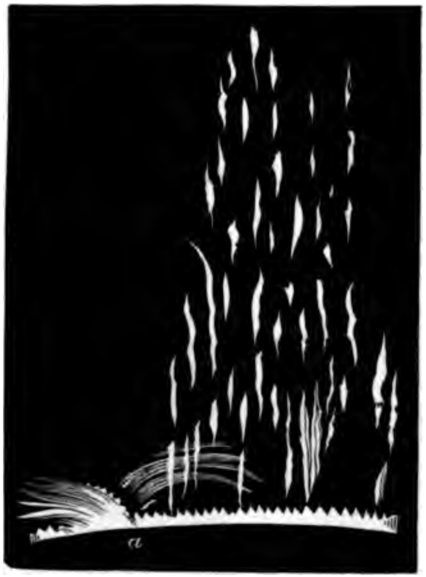

Fig. 136.—The prominence after the "explosion."

"What was my surprise, then, on returning in less than half an hour (at 12.55), to find that in the meantime the whole thing had been literally blown to shreds by some inconceivable uprush from beneath. In place of the quiet cloud I had left, the air, if I may use the expression, was filled with flying *débris*—a mass of detached vertical fusiform filaments, each from 10″ to 30″ long by 2″ or 3″ wide, brighter and closer together where the pillars had formerly stood, and rapidly ascending.

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[1] The sketches do not pretend to accuracy of detail, except the fourth; the three *rolls* in that are nearly exact.

Upward rush of filaments,

"When I first looked some of them had already reached a height of nearly 4′ (100,000 miles), and while I watched them they rose with a motion almost perceptible to the eye, until in ten minutes (1.05) the uppermost were more than 200,000 miles above the solar surface. This was ascertained by careful measurement; the mean of three closely accordant determinations gave 7′ 49″ as the extreme altitude attained, and I am particular in the statement because, as far as I know, chromospheric matter (*red-hydrogen* in this case) has never before been observed at an altitude exceeding 5′. The velocity of ascent also, 166 miles per second, is considerably greater than anything hitherto recorded. A general idea of its appearance when the filaments attained their greatest elevation may be obtained from Fig. 136.

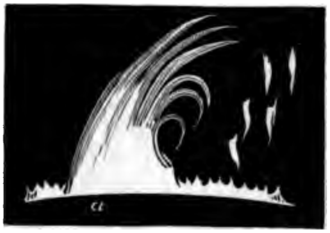

FIG. 137.—The "thunder-head" at 1.40.

which fade as they ascend.

"As the filaments rose they gradually faded away like a dissolving cloud, and at 1.15 only a few filmy wisps, with some brighter streamers low down near the chromosphere, remained to mark the place.

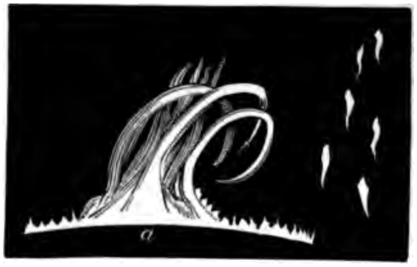

FIG. 138.—The "thunder-head" at 1.55.

"But in the meanwhile the little 'thunder-head,' before alluded to, had grown and developed wonderfully, into a mass of rolling and ever-changing flame, to speak according to appearances. First it was

> crowded down, as it were, along the solar surface; later it rose almost pyramidally 50,000 miles in height; then its summit was drawn out into long filaments and threads which were most curiously rolled backwards and downwards, like the volutes of an Ionic capital; and finally it faded away, and by 2.30 had vanished like the other. Figs. 137 and 138 show it in its full development; the former having been sketched at 1.40, and the latter at 1.55."

Young's observations.

We begin with a height of 200,000 miles, and get that entirely blown to nothing in a matter of a few minutes! A very important point remarked by Professor Young in this prominence, is new. You may recollect I mentioned that the velocity of the solar winds, determined by the changes of the wave-length, was something on the average like 120 miles a second—not an hour, but a second; and that the ordinary velocity of the uprush, was something like forty miles a second; but this prominence was absolutely seen visibly by Professor Young, to rise with a velocity of 166 miles a second. On this questions arise which I have not time to enter into, but at all events I think we may take this observation of Professor Young's as entirely endorsing and depoetizing,

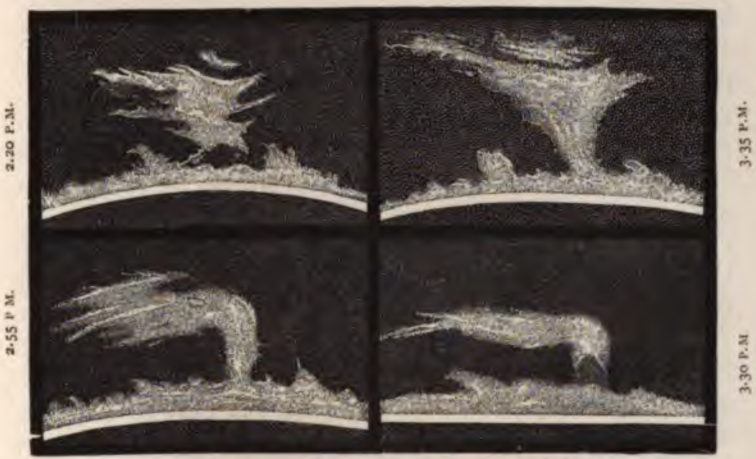
2.20 P.M.
3.35 P.M.
2.55 P.M.
3.30 P.M.

Fig. 139.—Changes in Prominences, Oct. 7, 1869. (Young).

A prominence rises 166 miles per second.

if I may so speak, the description which I ventured to give you of a very similar phenomenon which had been seen by myself.

And then, again, as to the changes: What are the causes of these enormous eruptions or explosions? call them what you will—these phenomena which seem perfectly overwhelming and bewildering in their vastness. This, of course, includes the question, What is a prominence? Now on this point there are several schools of opinion, so to speak. For instance, Professor Spörer and Professor Respighi are inclined to attribute them to electricity. Others maintain that they are simply due to heat: that is to say, that you get a certain amount of heat changing place in the atmosphere of the sun, and the changing of the place of the heat is accompanied by a change of the place of visibility, which enables us to determine the existence of the vapours; while others, again, suppose that the prominences are really ejections from the sun, or if you like to call them so in extreme cases, absolute *explosions* below the niveau of the photosphere.

What is a prominence?

Views of Respighi and Spörer.

With respect to this question, I may remark that some time ago, I think about April 1870, I was very much astonished to see very near the limb of the sun, when I was examining the F line, first of all, that the bright chromospheric line not only intruded very much more than it generally did on to the disc, but that I had close to it a phenomenon with which I was perfectly familiar,—which I had been induced to believe in observationally—although absolutely I knew nothing whatever about it, and therefore had no theory on the subject. I saw suddenly a lozenge of light, as I called it, due, I hold, to high-pressure bright hydrogen, the dark hydrogen absorption appearing over it, although the absorption of the cool hydrogen had been entirely obliterated nearer the sun's limb. Well, I gradually watched this lozenge, if you will allow me still to continue to call it so, until it travelled

The "Lozenge"

very nearly to the edge of the sun, and before it got quite to the edge of the sun, close on one side of it, I saw what I had never seen so decidedly before, a tremendous alteration of wave-length in the F line,—the F line being deflected first very violently to the violet, and then as violently to the red end. Also, in the same locality, I saw the F line broken into two parts—it was doubled. And what was going on while this was happening? A prominence, obviously with its root some distance from the limb, had gradually travelled beyond the limb; in appearance it became very much more elevated, and seen, as it were, in perspective over the limb; but what I saw first was very rapidly changed, in a way that would be explained by supposing that cyclones were being shot up into the solar air like bombs! the changes in the F line were so rapid and curious. I was not observing with an open slit, so I at once coined the term "motion forms," because the forms observed did not in any way represent the shape of the prominences. But the extreme velocity can be imagined from the great departure of those bright lines from the stable dark line F, seen below them (Fig. 149); and not only that, but we can think out the explicit character of this prominence action. They were really in this case, as already stated, smoke rings thrown up by enormous circumsolar action. There are indications of enormous motion in one direction, and there are enormous motions in another. I watched the phenomenon for a considerable time, and then placing the slit tangentially over the root of the prominence which had travelled nearly to the limb, I saw that instead of one lozenge, as we had to start with, we really had three.

Behaviour of F.

"Motion forms."

Now, what does that mean? Bearing in mind that in all that has been stated we have associated the thickening or thinning of a line equally on both sides with an increase of pressure, you will see that in this lozenge we have a very fair indication of an actual uprush of the high-pressure hydrogen from the photosphere. And

A lozenge indicates high pressure.

the fact that it is hydrogen and nothing else which is thus emitted, is, I think, rendered evident by the fact that only in the hydrogen lines did you get this extreme widening—this lozenge form, which is a *lozenge* and not a *line*, because the high-pressure hot hydrogen has heated and brought up with it hydrogen less dense, lying so to speak, on the margin of the solar crater. So that I think that so far as this fact—this series of facts—which I have ventured to bring before you goes, if my explanation is correct we have evidence for saying that for the case of this prominence, at all events, it was due to an uprush of high-pressure hydrogen; and I should tell you at the same time that this lozenge was connected with a facula.

The lozenge connected with a facula.

THE FACULÆ.

Distribution of the faculæ.

A most cursory glance at the sun shows us that the faculæ lie generally over the sun; that they are not confined to two main zones, like the spots, although it is quite true that they affect the spot-zones in the same way that they generally form walls round the spots. We know that we have indications of less absorption in the case of the faculæ than we have in the case not only of the spots, but of the general surface of the sun; so that we have, so to speak, faculæ at one end of the scale and spots at the other; the general surface of the sun occupying the interval. The selective absorption increases from the spectrum of the faculæ to the spectrum of the deepest spot that you can get; and if you will think the matter over a little, you will find that this will at once account for the fact that the faculæ are so much better seen near the sun's limb than they are anywhere else, for although it is perfectly obvious to any one who considers the question, that the faculæ must be as present in the centre of the sun's disc as they are at the limb, they are best seen at the limb. The change in the amount of general absorption between the spectrum of a facula and the spectrum of the general

Why they are best seen at the limb.

surface of the sun is so small that it requires magnifying, so to speak, before it becomes apparent, and this magnification we get at the edge of the sun, where a greater thickness of atmosphere is brought into play.

Hence we should hold the faculæ to be the higher cloud-domes, so to speak, of the photosphere. Since I last addressed you, they have been stated to be nothing but the permanent roots, so to speak, of prominences, and I shall have more to say about this by-and-by. But there is one point of great interest, namely, that the idea which has been started that the brightness of the faculæ was due to the presence of bright prominences on the sun, and therefore associated with the presence of bright lines in the sun, has, I think, been entirely negatived, although I confess all the observers of these phenomena are not yet content to see in the brightness of the faculæ merely a decrease in the general and selective absorption, which at times is so great that in the spectrum of a facula you see very often very many Fraunhofer lines of the solar spectrum disappear. The absence of continuous absorption, to which I strongly hold, has not sufficiently been brought forward among the evidence. Of course I do not affirm that bright lines are always absent, I only hold that reduced continuous absorption is always present. The time has now arrived when the distribution of the faculæ should be carefully studied, and I believe Father Secchi has already begun this branch of research.

Brightness of faculæ due to decreased absorption.

THE BASE OF THE SOLAR ATMOSPHERE.

Having thus dealt very briefly with the new observations and ideas connected with spots, prominences, and faculæ, we are now in a position to consider the present state of our knowledge concerning what we may call the base of that part of the solar atmosphere above the upper level of the photosphere. You may recollect that in my last lecture I stated that Dr. Frankland and myself had come to the conclusion that the existence of any such extensive corona as had been drawn by previous observers,—coronas extending beyond the sun two or three solar diameters, was extremely improbable, and our reasoning was based upon these facts—facts as they then appeared to everybody. First of all, that hydrogen seemed to be the only element normally present in those circumsolar regions, then being examined with all the freshness of novelty, the strange yellow line D_3 being considered as a hydrogen line, which for some reason or other we could not detect in our terrestrial hydrogen. Another idea was that the pressure was extremely small; another fact to be borne in mind was that hydrogen was the lightest gas known to us—the element with the least atomic weight, so that it seemed extremely improbable that anything could lie outside it; and I told you by the way, and it shows you how careful one ought to be in all these things, that a certain line—that numbered 1474 on Kirchhoff's scale—which had been seen by the Americans in the eclipse of 1869, was one of the most extraordinary indications we then had of the eruption of iron vapour to a considerable height in the prominences. Now, here the progress which we have to record is truly enormous. In the first place we begin with a low chromosphere, low pressure, and simple composition; and I may say broadly that we end with high chromosphere, high pressure, and most complicated composition. In the first place, we are perfectly certain now that the line D_3 has nothing in the world to do with hydrogen. On this head, in addition to the evidence furnished by Professor Young, to which I have already called your attention, and to other experiments of my own and Dr. Frankland's, I will refer to our eclipse work in India, which will at once convince you that the line D_3 has nothing to do with the hydrogen. In Professor Respighi's observations which I hope to have time to explain to you more fully,[1] I will call your attention to the four arcs, one red, another yellow, another green, and another violet.

First notions.

Enormous progress.

D_3 does not belong to hydrogen.

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[1] See p. 374, where these observations are discussed at length.

These are the arcs seen by Professor Respighi, formed by the prominences and chromosphere which was about to be eclipsed. Now consider the three large rings, two of them mere continuations of two of the arcs, the one the red, the other the blue, the green ring being an interpolation—of which more presently. I wish to point out that if the yellow line were as much part and parcel of the hydrogen spectrum as the red line and the blue line, we should have had here a yellow ring as well as the red ring, and the blue one; but you see that the yellow ring has entirely disappeared, and the reason of that is that the hydrogen absolutely physically present in the sun on the 11th of last December did not give us that yellow line; and therefore, I think, we may look upon that as the *coup de grace* to the notion that the yellow line is due to hydrogen in any solar condition whatever.

Secchi's continuous spectrum layer.

Soon after the first discovery of the materials of which the new stratum round the sun was composed, it was stated by Father Secchi that separating altogether the hydrogen from the photosphere was a region *in which there was no hydrogen, but which gave us a continuous spectrum.* Now all the work which had been done by everybody, excepting Father Secchi, who had taken up the question, went entirely in favour of the notion of the perfect continuity of the solar atmosphere from the highest point we could get at to the lowest point we could get at; and therefore I objected strongly to this stratum, whether it gave us a continuous spectrum or whether it did not, because I could see the lines of hydrogen absolutely continuous up to the photosphere, and in the spots, so to speak, down to the bottom, because occasionally in the spots you got hydrogen lines thickened, although that usually is not the case. Hence I objected to this interpolation, considering that what Father Secchi called a real continuous spectrum might probably have been an apparent one. As I remarked at the time, there is evidence of much reduced selective absorption, and

Objections.

some of the dark lines not altered in the facula spectrum even might be obliterated altogether because the slit in these delicate observations embraces both photosphere and chromosphere with their complementary spectra. The amount of change in these depend upon the complexity of the vapours in the part of the chromosphere observed, and I had obtained a great number of bright lines in the stratum of extreme thickness overlying the photosphere, in observations made about that time. It was stated by myself that there certainly was a great decrease in the darkening of a great many of the lines when you came to examine very delicately the fine region close to the photosphere; but others, as well as myself, were disinclined to agree to a *true* continuous-spectrum-giving stratum there, and my opinion is still unchanged although observations since have increased the number of lines at the base of the chromosphere, and therefore increased the number of elements there, and therefore increased the pressure in this region.

Gradually increasing number of chromospheric lines.

Let me just mention one or two of these observations. In the first place, I was not long before I got a list of some 30 or 40 lines seen in this region. Professor Young shortly afterwards in the drier, and therefore for this work much finer climate of America, gave us a catalogue of 103 lines—a catalogue of extreme interest and value—in which he has for the first time, I may parenthetically remark, detected the possible association of other bright lines with the strange line D_3, which appear to be the representatives of some unknown material which is at present beyond our ken. Then, I myself, first of all, got a complete welling-up of magnesium and iron with a great many lines reversed all round the sun, as I have before mentioned; and then in a sharply defined brilliant prominence it looked very much as if all the lines in the spectrum were reversed. Next, during the totality of 1870, the reversal of nearly all the Fraunhofer lines was observed by the distinguished American astronomer, Professor Young, and Mr. Pye—a young Englishman, who

Reversal of nearly all the Fraunhofer lines during the eclipses of 1870 and 1871.

was working with him. In the eclipse of 1871, the same thing to a certain extent was made out, at Bekul in India where I was, by Captain Maclear, who was armed with a very fine spectroscope; and the spectrum of the base of the chromosphere was very carefully watched, as it gradually retreated, so to speak, along the limb of the sun, as totality came on. Under these conditions we certainly saw a very large number indeed of solar lines, extremely faint, and extremely short, but still bright, and not all coming up to the same height. I may mention that all the Indian observers did not get out this fact; but still, taking the evidence as it stands, I think that the Indian observations, taken in conjunction with all the others, show there is a real increase in the number of lines in these lower regions as this lower stratum, which rests on the photosphere, is approached, and the hollows of which are doubtless filled with denser vapours still, for, of course, in the hollows and close to the exterior of the photosphere must the reversal of the dark lines be effected.

Zöllner's theory.

Do we know anything more of the photosphere itself? Nothing certain, but I may here mention but very briefly an idea thrown out by Professor Zöllner, that in the photosphere there is a liquid layer some eight seconds below the upper niveau of the photosphere; and he attributes to this the dark appearance of the nuclei of spots, and looks upon it as the region where all the pressure is put on to the hydrogen, after escaping which it bursts out in the tremendous way Professor Young and others have seen. But, unfortunately, if you examine Professor Zöllner's paper carefully, you will find that he has taken necessarily as data in his calculations, a great many of the facts which probably now will require a certain amount of reconsideration. For instance, he has taken the pressure at the top of the photosphere as extremely low. Now I myself should not now be prepared to admit that extremely low pressure.

Of course the region we are now considering may not only be regarded as the base of the prominences, but as that in which the spot phenomena, if not quite are very nearly carried on; and it may happen—I throw this out as a suggestion—that we shall find that as the spectra of the spots change from cycle to cycle—I have already told you the extreme difference in the spectrum of the sun-spots, as seen at different times—we may find that the same cause which is at work in raising and lowering the niveau of the spots, and in producing the welling-up of the magnesium and iron, may have something to do with this extreme complexity of the chromosphere which has lately been determined, and it may be found that it will change from time to time. I may here refer to a photograph which I was able to take a few days ago of a part of the spectrum, which includes the two lines H—H^1 and H^2;—and although I do not at all state this connection as a final one, I think you will see that if we can associate any of the changes in the thickening of the lines with any of the changes in any way resembling a cyclical change in the sun, photography will prove a very valuable ally. If we compare the thickness of the two H lines in the photograph taken by Mr. Rutherford some years ago, with the thickness of the same lines as they were photographed only a few days ago, we find a considerable difference which I do not think arises from photography alone. You will acknowledge the extreme importance of photographing the spectrum, from what I remarked about the research generally, namely, that as we go on, we get more and more into details; we leave behind the region of bold lines and high lights, to come down to minute work and more delicate shades. So in the case of solar spectrum it is no longer a question of the spectrum, it is a question of a line, and much now hangs upon the position and upon the record of any one line in the solar spectrum. When I tell you that in Ångström's map of the spectrum, which is certainly the best and most laborious map which has ever been made, between

Change in width of spectral lines.

these two very dark lines on the photograph, three only are recorded where the photograph shows 43, you will recognize at once the importance of the new kind of record, in which if there is a change in a series taken, say every three months, such change cannot escape detection.

THE LIMIT OF THE SOLAR ATMOSPHERE.

Equal progress on this point.

We will next carry our inquiries to a somewhat higher region—to that region of the sun's atmosphere, namely, which lies above those to which I then referred—the region of the corona. It will not take me very long to convince you that in the matter of our knowledge of the limit as well as of the base of the solar atmosphere, our progress since I last addressed you has been enormous. You may recollect that I told you some three years ago that the Astronomer Royal, and a German Astronomer, who carries almost equal weight in his own country—I refer to Mädler—after they had both of them viewed the eclipse of 1860, and not only had seen the eclipse themselves, but had had an opportunity which they had largely availed themselves of, of studying all the observations made by the other astronomers who were there assembled, they, I say, came to the conclusion that the corona was a compound phenomena, part of it being undoubtedly solar, as everybody, of course, was willing to admit, seeing that part of it had been seen time out of mind even before the sun was eclipsed; but that part of it also was in their estimation not solar. Up to the eclipse of 1860, we had not that truly unmistakeable record of the corona which we have now. For instance here, again, is the picture of the corona seen in the eclipse of 1858, which I brought to your notice three years ago with the remark that one part of it was undoubtedly solar, but that about certain other parts there was still some considerable doubt. I next showed you the corona visible round the eclipsed sun

The corona a compound phenomenon.

Coronas of 1858 and 1860.

in the year 1869, in America. In this we saw something different; and here, in fact, has been the difficulty in all these observations, that whether we deal with the flux of time, from eclipse to eclipse, or whether we deal merely with those differences which arise from change of station, we get such enormous changes in the pictures which are presented to us, now by one eclipse, now by another, now by one observer, and now by another, of the same eclipse. From these causes the thing is very much more difficult than the ordinary beholder would imagine that it could be. In the drawings made by Plantamour in 1860, we find another line of evidence, namely, that actual changes in the corona were observed to take place during the eclipse. You may recollect that I told you that the result which Dr. Frankland and myself, in the laboratory, had arrived at, seemed to entirely agree with this line of thought. We did not quite see how with the low pressure which we had determined, on the assumption of a simple composition of the chromosphere, there could be any very large corona outside it; that there was some corona outside was undoubted. This might consist of the hydrogen obliterated generally by the illumination of our air, but that any very large corona existed outside seemed very doubtful. What then is the present view? what the change from 1870 to 1872? We have discovered that there *is* something outside the hydrogen; that the chromosphere as you know, is more complex; that the pressure therefore is greater; and we have photographed a large corona which we know to be solar. So that unless some enormous change is going on in the outer limits of the solar atmosphere from year to year, or from sun-spot maximum to sun-spot maximum, the corona which has now been undoubtedly located at the sun by the observations we were fortunate enough to make in India; the corona, I say, is certainly very much larger and takes a higher place in the solar economy than was formerly imagined. I may remind you that since the beginning of 1870 there have been two eclipse expeditions—one to Sicily

Changes in the corona during an eclipse.

Main result of the work.

and Spain, in December 1870; another to India, in 1871. Unfortunately, as most of you know, the weather in 1870 was very bad indeed. The important fact determined in 1870 by Professor Young was connected with the line 1474, the American observations of 1869 being endorsed and extended. It is the study of this line which has taught us the existence of some unknown element extending further from the photosphere even than hydrogen. That it is not iron, as was at first supposed, I have lately determined in a series of researches now about to be laid before the Royal Society.

At the same time that this line was observed to extend to a distance of 20′ from the sun, the lines of hydrogen were observed eight minutes above the sun; this was supposed to be due to reflection, or some similar cause. A comparison of photographs, taken in Sicily and Spain, seemed also to indicate in the apparent boundary of the corona, dark spaces called rifts, which have been acknowledged to be identical in the two photographs: this part of the corona was therefore solar. Now in the observations of the last eclipse we have determined that these rifts really represent, as it were, indentations into the solar atmosphere, reaching at times to a considerable distance. Moreover by a new method of research we were able to prove that hydrogen really exists to a height of some seven or eight minutes at least above the sun, far above the vividly incandescent hydrogen which we can see by the new method without waiting for an eclipse; so that this after all is only the base, a small thin layer at the base, of an enormous envelope of hydrogen, probably on the average twelve minutes high, according to the photograph, if we accept the photograph as belonging to the hydrogen as well as to any other materials. And this raises an important question: while in the photograph the edges of the corona are seen quite jagged, the limit of the corona as indicated by the spectroscope was perfectly regular. There, again, is great food for thought, and it is a question about which we shall be able to speak with much

Hydrogen higher than was imagined.

greater certainty than it is possible to speak now, after the experience of two or three more eclipses; and we shall probably find it is associated with phenomena connected with a possible reflection of solar light by the materials of which the solar atmosphere is composed. I can only very briefly allude to this point, but on it the verdict, I think, is now final, although science has been oscillating first one way and then another now for a good many years on the subject. Dr. Janssen, the eminent French observer, who was observing in India not far from one of our stations, was fortunate enough to detect in the spectrum of the corona not only bright lines, as we all of us had done, but dark lines which could only have been due to sun light reflected by something in the corona, and the polariscope also indicated much reflection.

Detailed study.

Now then, accepting these photographs, and accepting these spectroscopic and polariscopic observations as giving us some just notion of the sun's outer atmosphere, let us look at it a little more in detail. In the first place we have, let us say, twelve minutes high all round the sun a boundary more or less jagged, more or less circular, as you will—that will depend upon whether you accept the photograph as representing the truth of nature, or the spectroscope. We have then an atmosphere twelve minutes high, the outer portion being composed of something about which we know absolutely nothing whatever, except that it gives us a line in the green which independent researches have shown is not an iron line. Let us call that x′—you will see why presently. Then we have also round the sun, at a height of, let us say, eight minutes, hydrogen. But here we must pause somewhat, and divide our hydrogen into layers. We may undoubtedly divide it into two strata. Here you see we approach the region and somewhat, therefore, the language of geology; and probably we ought to divide it into three strata, but let us be content with two. We have the hydrogen sub-incandescent, which we cannot see by the new method, and the hydrogen incandescent which we can, as its temperature is sufficiently great to make it excessively brilliant, so that we see it without waiting for an eclipse at all. Let us place this two minutes high. Below this, then, we have the yellow line giving substance, which is mixed up with the lower hydrogen; which is very rarely seen above the hydrogen, but which is often seen low down, in such a way that we are perfectly satisfied it is not hydrogen. Let us call this x. This probably may be placed one minute high. Then, from the observations on the base of the solar atmosphere, we have magnesium, and sodium, then barium and nickel, then iron, and a host of other substances, and travelling down from x′, which gives us the 1474 line and exists at the extremest, the most utterly distant, parts of the corona, right down through the solar atmosphere to the bottom of the deepest spot, we shall pass very much through the different substances in this order. Beginning with the 1474 element, we pass through the sub-incandescent hydrogen; deeper still we get to the incandescent hydrogen; then we go through the D_3 element; then we get into regions where the lines are generally mixed rather more together, but from which magnesium and sodium are generally ejected more frequently and higher than any other material; then we get into the more doubtful zone of barium and nickel, sometimes sodium being thrown up, sometimes barium, sometimes nickel; and then we come lower down into what may be called, so far as we shall ever be able to investigate the sun, the very bowels of our central orb, where we are certain to get iron, and we may get many other materials.

Succession of elements in the sun.

Structure of corona.

I will finish the picture of the exterior portion of the solar atmosphere by referring to its structure. On the last occasion I showed some photographs of prominences which I had sketched, and I told you that the region of the prominences in the sun, reminded one almost, now, of a forest of banyan trees, and again, the chromosphere put on the appearance of an English hedge-row, the prominences looking like luxuriant elms. That definition still, to my mind holds perfectly good. The structure is filamentous. When a disturbance is not going on, instead of getting the eruptive form, in which the prominences are straight and dense, we get a nebulous appearance. Now the whole phenomena of the corona may be defined in two words, *cool prominences.* I examined the corona with first-rate optical power, and under first-rate atmospheric conditions; I examined a large arc of it, and there was no part of the corona which I saw, which could not have been described as cool prominences; but in one part of the corona the line form, so to speak, predominated, and in another part the coronal masses got more and more to resemble those strange conglomerations which we see not only in some of the prominences, but also in some of the brighter nebulæ, so much so that although I was not very much excited, I still did exclaim, perhaps more loudly than I need have done, that the corona looked like the nebula of Orion; and there was one especial part of the corona which reminded me most vividly, even amongst all the excitement of those two minutes, of that exquisite drawing made by the late Sir John Herschel of the nebula round η Argûs. Passing then from one part of the corona to another, we still remain in the region of cool prominences, but in one part we get the nebulous prominences represented, and in the other we gradually put on the jetty appearance, so often chronicled by Respighi.

Corona, cool prominences.

Its nebular appearance.

THE CHROMOSPHERE.

You may ask, What is the relation of the chromosphere to this solar atmosphere which we are now discussing? My reply is that that is a question of definition. What we have to do now we know that the photosphere floats in the solar atmosphere, is to define the region above the photosphere. In the early days when the name was given, it looked as if hydrogen alone or almost alone extended above the photosphere, and the name was given to denote the bright line region in which a new world of phenomena was daily revealed to us by the new method. We now know that out of the reach of the new method there is a region of cooler hydrogen and something else—what that *else* is we do not yet know—but we have now to take it into consideration. I proposed, therefore, that the term photosphere should hold for all the solar material outside the chromosphere, as its continuity was undoubted, leaving the word corona for the mixed phenomenon, which we *see* when we can see it—that is, during total eclipses. But both Janssen and Respighi have considered this nomenclature inadequate, and propose to restrict the term chromosphere to the solar surroundings visible by the new method, naming the exterior portion *atmosphère coronale* or *chromosphère exterieure.* I willingly yield, with the remark that the last of these terms seems to me to be the better. Accepting this nomenclature, then, we have—

Revised nomenclature.

- Solar atmosphere
  - Chromosphere
    - Exterior.
    - Interior.
  - Photosphere

Interior chromosphere.

What, then, is the interior chromosphere? It is undoubtedly a region which as a rule bounds the convection currents of the sun; it is a region where there is a sharp break in temperature: its hairy or cloudy outline probably depending somewhat upon the upper incandescent air.

THE MOVEMENTS OF THE SOLAR ATMOSPHERE.

I have now to pass as rapidly as may be to another increase of our knowledge—as great as that we have obtained from those two eclipse expeditions,—I refer to some magnificent work done in the clear sky of Italy by Professor Respighi, which has taught us more about the movements of the sun's atmosphere, by means of the new method, and in a couple of short years, than we could probably have learnt without it in thousands of centuries.

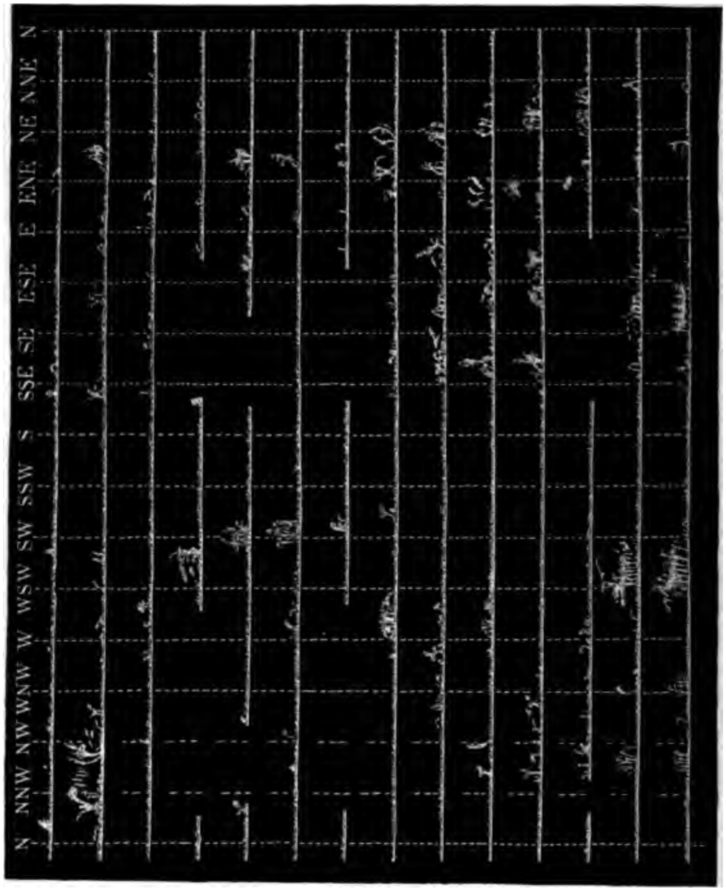
What do we know already about the sun's atmosphere? In what has already been done we are limited especially to the base of that atmosphere. We have been limited, so far as a complete and careful study of it is concerned, to the phenomena of the spots. We may say roughly that the salient phenomena of the spots are something like these: sometimes we have a great many spots in the sun, sometimes we have very few: the time from a minimum to a minimum or from a maximum to a maximum is somewhat over eleven years, but there are longer periods still.

Spot zones.

But this is not all that astronomers have been able to tell us about the distribution of the spots. The work of seven long years is recorded in Mr. Carrington's book on the sun, and his plates show very clearly a fact that was gathered by the first discoverers of the spots, namely, that they affect certain regions. Let us draw a diagram of the sun, and represent on it the solar equator and the parallels of thirty north and south latitude. As the time of minimum sun spots—when there are no spots or very few on the sun—passes by, what happens? We begin to see the spots putting in an appearance at about latitude thirty north, and thirty south. As the maximum is more nearly reached they gradually approach the equator; the zone widens, and the gradual increase in the range of the spots becomes very apparent: the spot zone, that is to say, gets wider as it gets nearer the equator. We have therefore two spot zones—one to the north, and the other to the south of the equator; the amount of spot frequency increases rapidly, and at the same time the zone in which the spots mostly appear varies considerably. Probably future investigation will show that these zones are not absolutely symmetrical with the sun's equator. Here, then, we have roughly the outcome of a great many years of work on the sun's spots—those sun-spots lying at the base of the solar atmosphere. Now it is clear to you that if we could get anything higher up in the solar atmosphere, (where the pressure must be much less, and where we know that the changes are much more rapid) to render evident to us exactly what is going on there; and if from these records we found that there is a method in all the apparent irregularity; it is obvious that we should have in this way a much greater chance of being able to get bodily into the secrets of these solar regions than we should in any other way. If in the region of the spots it is extraordinary for a spot to change very much from day to day, and if in the region of the prominences it is extraordinary if they do not change from hour to hour, it is evident that a very little labour will go a great way towards showing us first of all whether there is a law, or whether there is not a law, which regulates the apparent irregularity with which the prominences make their appearance; and if there be a law, letting us know what that law is.

Respighi's work.

Now, pre-eminent amongst the men who have worked at this branch of research, Professor Respighi must be mentioned. I shall give you some idea of his wonderful assiduity when I tell you that since the discovery of this new method, he has already mapped more than 8,000 prominences, and when we come, as some of you do, to know exactly what that means; that every day, or sometimes two or three times a day, you have to bring the sun's image on to the slit-plate of your spectroscope, and then carefully go round the whole limb of the sun bit by bit, carefully recording the position and form of all the prominences, be they big or be they little; most carefully measuring their heights at the same time; it will be perfectly clear to you that a labour of this kind is one of enormous magnitude. I have here a diagram which will show you the way in which such observations are recorded. The prominences may be mapped on a circle, representing the profile of the sun, with the north, south, east, and west points and the position of the poles of rotation ; or they may be mapped along a line in each case beginning at the northern point of the sun, inserting the prominences at the proper degree, and then afterwards noting the exact position of



the sun's equator and poles. In this graphic way the history of every prominence is written down from day to day, if the weather be fine. You have now an idea of the enormous work which has been done by Professor

Respighi. And what then is the result of Professor Respighi's labours? Of the 7,449 prominences which he observed from October 26, 1869, to the 5th of May of the present year—he found that 1,330 are below one minute high, 1,150 between one minute and two minutes, and so he goes on till he finds only five exceeding five minutes in height.

Frequency of prominences.

Then with respect to the frequency with which these prominences make their appearance. This part of our knowledge is to be got from Professor Respighi's memoirs in two different ways. We have first of all the numbers of the prominences taken together, big and little; and we learn from these what has happened with reference to the whole number of prominences visible. But in addition to this he had sorted out the more important prominences, the larger, fundamental, ones; and we have the information tabulated again with respect to them. In these diagrams[1] I have very roughly, but I hope sufficiently, shown you by means of zones, what Professor Respighi has found with reference to all the prominences on the sun's disc. His observations began in 1869. I will read you the exact periods. The first began October 26th, 1869, and extended to the 30th of April 1870. The second period was from the 1st of May to the 30th of October 1870; the third period is from the 1st of November 1870, to the 30th of April 1871; and the fourth period is from the 1st of May 1871, to the 1st of September 1871.

Prominence curve.

Now what makes these diagrams so extremely important is the fact, namely, that they show what is going on in the prominences as we approach the maximum period of sun-spots; we get the prominence story while the sun-spots are working to their maximum. Now, what is that story? I have before told you that the rise of the sun-spot curve, is very much more rapid than its fall. Therefore, in May 1869, we were further from the sun-spot maximum than we were a year after the curve had turned. What do we find? The equator being represented by this centre line, let us examine the diagram containing the results of the first period of observation. North and south of the equator we get two zones, representing a large number of prominences, which die away towards the two poles. The places where there are no prominences are the equatorial regions and the two poles. We pass on to the next period, and observe what a change in a few months! The equator here is perfectly blocked up with prominences. The well-defined maxima have disappeared; there are no longer prominence zones, there is an enormous prominence belt. I should tell you this does not represent the map of the phenomena with final accuracy, it is simply a rough sketch of what happens, and I have put the facts in their strongest form. In the northern hemisphere there is a distinct massing of prominences which we do not find in the southern hemisphere; at the same time both the poles are free from prominences. We now approach another period—a period just before the maximum of sun-spots. Here we find the equator open again, and with the open equator, as before, we get a return of the symmetry of the prominence zones; only, instead of two definite zones, as we had in the first period, we have four, and they are gradually creeping towards the poles. Here again, at the period of maximum of sun-spots we get the equator closed again, as it was before, and we also get the same lob-sidedness, so to speak, for there is a maximum to the south not represented at the north side of the equator, in exactly the same way as when the equator was closed before, there was a maximum at the north side of the equator which was not represented at the south; and there is another important point: the poles are not closed—the poles are now covered with prominences.

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[1] It has been found impossible to give these diagrams here, but they are not essential.

I think you will at once see that we are in the presence of a study which, if carefully carried on, cannot fail to tell us most precious truths with reference to the movements

Connection between solar and terrestrial meteorology.

in the solar atmosphere, and make us acquainted more accurately with the various cycles of solar activity. You all here are so interested in the movements of our own atmosphere, that I need not remind you that year by year, one might almost say day by day, the movements of our own atmosphere, from a planetary point of view, are necessarily intimately locked up with the sun's activity; so that the only proper way of studying meteorology is to begin, so to speak, at the fountain head, and study the sun. Mr. Meldrum, a distinguished meteorologist—who lives not in the temperate zones of the earth, where the meteorological conditions are irregular, but in the torrid zone, where regular meteorological phenomena, and among them cyclones, abound — tells us that it is no longer correct to merely associate cyclones with the tropics. He tells us that the whole question of cyclones is a question of solar activity; and that if we write down in one column the number of cyclones in any given year, and in another column the number of sun-spots in any given year, that there will be a strict relation between them; many sun-spots, many hurricanes; few sun-spots, few hurricanes. Only this morning I have received a letter from Dr. Stewart, who tells me that Mr. Meldrum has since found that what is true of the storms which devastate the Indian Ocean, is true of the storms which devastate the West Indies; and on referring to the storms of the Indian Ocean, Mr. Meldrum points out that at those years where we have been quietly mapping the sun spot maxima, the harbours were filled with wrecks, vessels coming in disabled from every part of the great Indian Ocean. Now that surely is something worth considering, because if we can manage to get at these things, to associate them in some way with solar activity, so that there can be no mistake about it, the power of prediction—that power which would be the most useful one in meteorology, if we could only get at it—would be within our grasp.

CHAP. XXVI.

It would be a curious inquiry which we commend to those learned in statistics, to determine how many millions of observations have been made in the British Isles on dry and wet bulb thermometers, on barometers, and on other meteorological instruments. It would be a still more curious inquiry, seeing that the infinite industry displayed in these observations shows that the importance of the study of Meteorology is universally conceded, to determine why it is that meteorologists, state-endowed and otherwise, have, as a rule, been content to grope their way in the dark, and not only not seek to find, but persistently refuse the clue, which, if followed, would bring them into the light of day. When some one some centuries hence—thank heavens, we have always that to look to in all branches of research—comes to consider the work done by meteorologists during the present century, he will, unless he be some patient German Dryasdust determined to examine all minutes of Boards of Visitors, all Kew Committee Records, and the like, give up the task in the most utter despair; on the whole perhaps this is the best thing that could happen.

Importance of cycle.

Surely in Meteorology, as in Astronomy, the thing to hunt down is a cycle, and if that is not to be found in the temperate zone, then go to the frigid zones or the torrid zone to look for it, and if found, then above all things, and in whatever manner, lay hold of, study it, record it, and see what it means. If there is no cycle, then despair for a time if you will, but yet plant firmly your science on a physical basis, as Dr. Balfour Stewart long ago suggested, before, to the infinite detriment of English science, he left the Meteorological Observatory at Kew; and having got such a basis as this, wait for results. In the absence of these methods, statements of what is happening to a blackened bulb in vacuo, or its companion exposed to the sky, is, for research purposes, work of the tenth order of importance.

I said the thing to hunt down is a cycle. Now it may be asked,—Is there anywhere on earth a weather cycle? but anyone who asks this question will at once answer it himself: the question would certainly suggest the trade winds and monsoons, which are short-period cycles. But is there anything more than this?

Cycle in Ceylon.

When I was preparing to go to India last year to observe the eclipse, Mr. Ferguson, the able editor of the *Ceylon Observer*, who happened to be in London, was good enough (he was good enough to us all afterwards, and the Eclipse Expedition of 1871 have much to thank him for) to give me much valuable local information about the time of the year at which the monsoons broke up in the island. Nor was this all; he added that everybody in Ceylon recognized a cycle of about thirteen years or so in the intensity of the monsoon—that the rainfall and cloudy weather were more intense every thirteen years or so. This of course set one interested in solar matters thinking, and I said to him, "But are you sure the cycle recurs every thirteen years; are you sure it is not every eleven years?" adding as a reason that the sun-spot period was one of eleven years or thereabouts, and that in the regular weather of the tropics, if anywhere, this should come out.

This conversation Mr. Ferguson thought fit to reproduce in the *Ceylon Observer*, and I have now lying before me a cutting from a number of that paper I saw in India (unfortunately it is cut too much, for both date and writer's name are gone),[1] from which I make the following extract:—

> "The period is not *thirteen* years but eleven (as Lockyer states it) In the tropics, or at least, here in Ceylon, where we enjoy the regula changes of the two monsoons, the basic period runs five or six years dry, and five or six years wet. These make *eleven*, and they form the medium cycle of *three*—the grand cycle of thirty or thirty-three years —being three periods of the eleven cycles. But I must premise here that though I adopt these figures as noting a general run of cycle, i is by no means to be expected that, always, these changes shall run with mathematical correctness in given grooves, for there may be thirteen at one time, and next eleven, giving a grand cycle of thirty or thirty-three years."

It will be seen, then, that those who are not professed meteorologists recognize not only the eleven-year period in the Ceylon rainfall, but possibly also a higher one still —that of thirty-three years. In the press of work that has fallen upon me since my return to England, after my three months' absence, I have been prevented from taking the opinion of my meteorological friends upon this most important matter; but now there comes evidence on the question from an authority whose facts and opinions at once settle the matter.

Mr. Meldrum's work.

Mr. Meldrum, of the Mauritius, to whom belongs the honour of having established that the number of cyclones in the Indian Ocean and the West Indies varies with the sun-spot area, has lately attacked the rainfall of the Mauritius, Queensland, and Adelaide from the sun-spot period point of view, with results which are simply startling although Mr. Meldrum very properly puts them forward to stimulate further inquiry, and not as final.

Mr. Meldrum's step from cyclones to rainfall is a very obvious one, because it is well known that cyclones are generally accompanied with torrential rains. The years therefore, in which cyclones are most frequent should be more rainy than the years in which they are less frequent But Mr. Meldrum remarks, in his paper communicated to the Meteorological Society of the Mauritius, "to make th rainfall a fair test of the existence of a periodicity of cyclones in the Indian Ocean it would be necessary to know the annual rainfall over the same area for the same length of time. If such rainfall had no periodicity, we should have reason to doubt a cyclone-periodicity; but if there was a similar rain-periodicity, it would, so far, be a confirmation of a cyclone-periodicity."

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[1] Thanks to Mr. Ferguson's kindness both these have been supplie since. See note at end of chapter.

Accordingly, as it is impossible to determine the rainfall over the ocean, the law of the cyclones of which has been approximately determined, there remains but one course open, to observe the rainfall on the nearest points of land. This is as follows for the above-named stations:—

BRISBANE.		ADELAIDE.		PORT LOUIS.	
Years.	Rainfall. Inches.	Years.	Rainfall. Inches.	Years.	Rainfall. Inches.
		1839	19·840		
		1840	24·107		
		1841	17·956		
		1842	20·318		
		1843	17·192		
		1844	16·878		
		1845	18 830		
		1846	26·885		
		1847	27·613		
		1848	19·735		
		1849	25·444		
		1850	19·274		
		1851	30·633		
		1852	27·340		
		1853	26·295	1853	39·829
		1854	15·346	1854	39·435
		1855	23·145	1855	42·665
		1856	24·921	1856	46·230
		1857	21·156	1857	43·445
		1858	21·522	1858	35·506
		1859	14·842	1859	56·875
1860	54·63	1860	19·670	1860	45·166
1861	69·44			1861	68·397
1862	28·27			1862	28·397
1863	68·82			1863	33·420
1864	47·00			1864	24·147
1865	24·11			1865	44·730
1866	37·24			1866	20·571
1867	61·04			1867	35·970
1868	35·98			1868	64·180
1869	54·36			1869	54·575
1870	79·06			1870	45·575
1871	45·45			1871	41·910

Now, we know, to start with, that the years of minimum and maximum sun-spot frequency were as follows :—

Min. epochs 1833, 1844, 1856, 1857
Max. „ . . . . . 1807, 1848, 1860, 1871 (?)

and Mr. Meldrum has shown that these years were also those of minimum and maximum cyclone frequency. Let us begin by examining the Port Louis Observations, embracing nineteen years (1853—1871).

Port Louis.

Taking the rainfall in each minimum and maximum epochal year, and in one year on each side of it, Mr. Meldrum gets—

	Years.	Rainfall.	Total Rainfall.
Min.	1855	42·665	133·340
	1856	46·230	
	1857	43·445	
Max.	1859	56·875	170·774
	1860	45·166	
	1861	68·733	
Min.	1866	20·571	120·721
	1867	35·970	
	1868	64·180	

"These figures show a marked excess of rainfall for the three years comprising the maximum sun-spot year (1860), which was also the year of maximum cyclone frequency.

"If in place of one year, we take two years on each side of the epochs, we shall get—

	Years.	Rainfall.	Total Rainfall.
Min.	1854	39·435	207·281
	1855	42·665	
	1856	46·230	
	1857	43·445	
	1858	35·506	
Max.	1858	35·506	234·281
	1859	56·875	
	1860	45·166	
	1861	68·733	
	1862	28·397	
Min.	1865	44·730	220·026
	1866	20·571	
	1867	33·970	
	1868	64·180	
	1869	54·575	

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Here, again, a similar result is shown. It is not so well-marked as the former one, partly owing, Mr. Meldrum suggests, to the rain-gauge having been removed in 1866 to a temporary Observatory, where the rainfall was probably somewhat greater.

> "So far, then, as the Port Louis observations enable us to judge, it may be said that during the last twenty years there has been a rainfall-periodicity corresponding with the cyclone-periodicity in the Indian Ocean south of the Equator.
>
> "This may be considered as confirmatory of the correctness of the cyclone period; for if the rainfall *at one station* shows a corresponding periodicity, much more should *a mean of the rainfall at many stations* within the whole cyclonic area do so."

Mr. Meldrum next passes on to the Australian observations, remarking that, although Adelaide and Brisbane are a long way outside the area for which the cyclone period was determined, there also the rainfall tables seem to point to a similar periodicity.

Adelaide.

The *Adelaide* twenty-two years' observations give :—

	Years.	Rainfall.	Total Rainfall.
Min.	1843	17·192	52·900
	1844	16·878	
	1845	18·830	
Max.	1847	27·613	72·792
	1848	19·735	
	1849	25·444	
Min.	1855	23·145	69·222
	1856	24·921	
	1857	21·156	

By taking five-years periods we get :—

Minimum = 100·076 inches.
Maximum = 118·951 "
Minimum = 106·090 "

Brisbane.

We next come to twelve years' observations at Brisbane, for which science is indebted to Mr. Edmund McDonnell. Comparing them with the Mauritius observations for the same period, we cannot but be struck with a resemblance, which comes out still more forcibly when we take three-years periods, thus :—

Years.	Port Louis.		Brisbane.	
1860	55·166	142·296	54·63	152·34
1861	68·733		69·44	
1862	28·397		28·27	
1863	33·420	102·297	68·82	139·93
1864	24·147		47.00	
1865	44·730		24·11	
1866	20·571	120·721	37.24	134·26
1867	35·970		61·04	
1868	64·180		35·98	
1869	54·575	141·760	54·36	178·87
1870	45·575		79·06	
1871	41·610		45·45	

At both stations the epoch of minimum rainfall is coincident, or nearly so, with the epoch of minimum amount of cyclones, which is itself coincident with the minimum of sun-spots; and that at, or near, the maximum of sun-spots and cyclones, we have also a maximum amount of rainfall.

Mr. Meldrum's important paper concludes as follows:—

> "From what has been said it will, I think, be admitted that at least a case of *supposed* periodicity of rainfall has been made out, and that it is highly desirable that the matter should be further investigated. This can be done chiefly by long-continued observation under the same conditions as to locality, size of gauge, &c., and perhaps to some extent by ascertaining, if not the actual rainfall, at least the years remarkable for the comparative absence or abundance of rain in former times.
>
> "It should be remarked that some localities are probably much more favourable than others for showing the operation of a general law of this kind; for there may be local causes affecting the rainfall so powerfully as to completely mask the effect of a weaker but more general cause; and therefore it would be no proof of the non-existence of a connection between rainfall and sun-spots to show that the observations taken at such and such places were not in conformity with the supposed periodicity.
>
> "We should be inclined to think that the best mode of testing the matter would be to obtain records of observations carefully made for a long period in some of the islands of the Indian and Pacific Oceans, for example, far removed from the disturbing influence of Continents, and then to take a mean of all the observations.
>
> "The Adelaide and Brisbane observations would seem to indicate a rainfall-periodicity altogether independent of a cyclone-periodicity, both being apparently the natural consequences of one and the same law. But it would be rash to say more at present, and I should wish it to be understood that the object of this paper is simply to stimulate further inquiry."

Cape.

Since Mr. Meldrum's results have reached me, I have tested the Cape and Madras rainfall, to see if the same result is to be got from them, and with the following results:—

	Cape.	Inches.		
Max.	1847	22·4	68·6	
	1848	23·2		
	1849	23·0		
Min.	1854	20·0	63·9	
	1855	24·5		
	1856	19·4		
Max.	1859	36·7	91·2	
	1860	29·1		
	1861	25·4		
Min.	1866	19·2	62·0	
	1867	22·9		
	1868	19·9		
Max.	1869	32·3	62·3	For two years only.
	1870	28·0		

Madras

From the Madras observations at my disposal only one maximum and one minimum can be given:—

	Cape.	Inches.	
Min.	1843	41·0	125
	1844	45·0	
	1845	39·0	
Max.	1847	81·0	175
	1848	40·0	
	1849	54·0	

Surely here is evidence enough, evidence which should no longer allow us to deceive ourselves as to the present state of meteorology. A most important cycle has been discovered, analogous in most respects to the Saros discovered by the astronomers of old. Indeed, in more respects than one, may the eleven-yearly period be called the Saros of meteorology, and as the astronomers of old were profoundly ignorant of the true cause of the Saros period, so the meteorologists of the present day are profoundly ignorant of the true nature of the connection between the sun and the earth.

What, therefore, is necessary in order to discover the true nature of this nexus? Two things are necessary, and they are these. In the first place, we must obtain an accurate knowledge of the currents of the sun, and secondly, we must obtain an accurate knowledge of the currents of the earth. The former of these demands the united efforts of photography and spectrum analysis, and the second of these demands the pursuit of meteorology as a physical science, and not as a mere collection of weather statistics. When these demands are met—and in spite of the Mrs. Partingtons who are endeavouring to prevent this, they will soon be met—we shall have a Science of Meteorology placed on a firm basis—the Meteorology of the Future.

[NOTE.—The letter to which reference has been made was written by Mr. Tytler, as I learn from the *Colombo Observer* of January 30, 1873, which contains more facts bearing on the subject. Mr. Tytler lays great stress on the cycle of about thirty years, pointing out that visitations of the horrible leeches of Ceylon and vast landslips occur at this interval; that the Singhalese with their traditions, going back some 2,000 years, believe in an "Edorvore Kala" and a "Weyokala" of thirty years or so.

Mr. Meldrum's researches have been continued since the date of the article with unceasing assiduity, and he has communicated further results to the Royal Society of London.]

PART II.

COMMUNICATIONS TO THE ROYAL SOCIETY OF LONDON AND TO THE PARIS ACADEMY OF SCIENCES.

I.—OBSERVATORY WORK.

FIRST PAPER.

Spectroscopic Observations of the Sun, No. I.—By J. N. LOCKYER, F.R.A.S. Communicated by Dr. SHARPEY, Sec. R. S. Received October 11, 1866.[1]

FIRST PAPER.

The theories of M. Faye and of Messrs. De la Rue, Stewart and Loewy.

THE two most recent theories dealing with the physical constitution of the sun are due to M. Faye and to Messrs. De la Rue, Balfour Stewart, and Loewy. The chief point of difference in these two theories is the explanation given by each of the phenomena of sun-spots.

Thus, according to M. Faye,[2] the interior of the sun is a nebulous gaseous mass of feeble radiating power, at a temperature of dissociation; the photosphere is, on the other hand, of a high radiating power, and at a temperature sufficiently low to permit of chemical action. In a sun-spot we see the interior nebulous mass through an opening in the photosphere, caused by an upward current, and the sun-spot is black, by reason of the feeble radiating power of the nebulous mass.

In the theory held by Messrs. De la Rue, Stewart, and Loewy,[3] the appearances connected with sun-spots are referred to the effects, cooling and absorptive, of an inrush, or descending current, of the sun's atmosphere, which is known to be colder than the photosphere.

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[1] *Proc. R.S.* vol. xv. p. 256.

[2] *Comptes Rendus*, vol. lx. pp. 89—138. See Chaps. 4 and 5.

[3] "Researches on Solar Physics." Printed for private circulation. Taylor and Francis, 1865. See Chap. 6.

Evidence in favour of the English theory obtained by the telescope.

In June 1865 I communicated to the Royal Astronomical Society[1] some observations (referred to by the authors last named) which had led me independently to the same conclusion as the one announced by them. The observations indicated that, instead of a spot being caused by an *upward* current, it is caused by a *downward* one, and that the results, or, at all events, the concomitants of the downward current are a dimming and possible vaporization of the cloud-masses carried down. I was led to hold that the current had a downward direction by the fact that one of the cloud-masses observed passed in succession, in the space of about two hours, through the various orders of brightness exhibited by *faculæ*, general surface, and *penumbræ*.

On March 4th of the present year I commenced a spectroscopic observation of sun-spots, with a view of endeavouring to test the two rival theories, and especially of following up the observations before alluded to.

First attempt with the spectroscope.

The method I adopted was to apply a direct-vision spectroscope to my 6¼-inch equatorial (by Messrs. Cooke and Sons) at some distance outside the eye-piece, with its axis coincident with the axis of the telescope prolonged. In front of the slit of the spectroscope was placed a screen on which the image of the sun was received; in this screen there was also a fine slit corresponding to that of the spectroscope.

By this method it is possible to observe at one time the spectra of the umbra of a spot and of the adjoining photosphere or penumbra; unfortunately, however, favourable conditions of spot (*i.e.* as to size, position on the disc, and absence of "cloudy stratum"), atmosphere, and instrument are rarely coincident. The conditions were by no means all I could have desired when my first observations were made; and, owing to the recent absence of spots, I have had no opportunities of repeating my observations. Hence I should have hesitated still longer to lay them before the Royal Society had not M. Faye again recently called attention to the subject.

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[1] *Monthly Notices*, *Roy. Ast. Soc.*, vol. xxv. p. 237. See *ante*, p. 24.

Spectroscopic appearance of a sun-spot.

On turning the telescope and spectrum-apparatus, driven by clock-work, on to the sun at the date mentioned, in such a manner that the centre of the umbra of the small spot then visible fell on the middle of the slit in the screen, which, like the corresponding one in the spectroscope, was longer than the diameter of the umbra, the solar spectrum was observed in the field of view of the spectroscope, with its central portion (corresponding to the diameter of the umbra falling on the slit) greatly enfeebled in brilliancy.

All the absorption-bands, however, visible in the spectrum of the photosphere, above and below, were visible in the spectrum of the spot; they, moreover, appeared thicker where they crossed the spot-spectrum.

I was unable to detect the slightest indication of any bright bands, although the spectrum was sufficiently feeble, I think, to have rendered them unmistakeably visible had there been any.

The absorption theory probably true.

Should these observations be confirmed by observations of a larger spot free from "cloudy stratum," it will follow, not only that the phenomena presented by a sun-spot are not due to radiation from such a source as that indicated by M. Faye, but that we have in this absorption-hypothesis a complete or partial solution of the problem which has withstood so many attacks.

The dispersive power of the spectroscope employed was not sufficient to enable me to determine whether the decreased brilliancy of the spot-spectrum was due in any measure to a greater number of bands of absorption, nor could I prove whether the thickness of the bands in the spot-spectrum, as compared with their thickness in the photosphere-spectrum, was real or apparent only.[1]

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[1] Irradiation would cause bands of the same thickness to appear thinnest in the more brilliant spectrum.

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On these points, among others, I shall hope, if permitted, to lay the results of future observations before the Royal Society.[1] Seeing that spectrum analysis has already been applied to the stars with such success, it is not too much to think that an attentive and *detailed* spectroscopic examination of the sun's surface may bring us much knowledge bearing on the physical constitution of that luminary. For instance, if the theory of absorption be true, we may suppose that in a deep spot rays might be absorbed which would escape absorption in the higher strata of the atmosphere; hence also the darkness of a line may depend somewhat on the depth of the absorbing atmosphere. May not also some of the variable lines visible in the solar spectrum be due to absorption in the region of spots? and may not the spectroscope afford us evidence of the existence of the "red flames" which total eclipses have revealed to us in the sun's atmosphere; although they escape all other methods of observations at other times? and if so, may we not learn something from this of the recent outburst of the star in Corona?

Can the spectroscope reveal the "red flames."

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[1] For a sketch of subsequent work on the Spot-Spectrum, see Note A.

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Brief Announcements.

Notice of an Observation of the Spectrum of a Solar Prominence, by J. N. LOCKYER, Esq., in a letter to the Secretary. From the "Proceedings of the Royal Society," No. 105, 1868. Received Oct. 20, 1868.

October 20, 1868.

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SIR,—I beg to anticipate a more detailed communication by informing you that, after a number of failures, which made the attempt seem hopeless, I have this morning perfectly succeeded in obtaining and observing part of the spectrum of a solar prominence.

As a result I have established the existence of three bright lines in the following positions:—

I. Absolutely coincident with C.
II. Nearly coincident with F.
III. Near D.

The third line (the one near D) is more refrangible than the more refrangible of the two darkest lines by eight or nine degrees of Kirchhoff's scale. I cannot speak with exactness, as this part of the spectrum requires re-mapping.

I have evidence that the prominence was a very fine one.

The instrument employed is the solar spectroscope, the funds for the construction of which were supplied by the Government-Grant Committee. It is to be regretted that its construction has been so long delayed.

I have, &c.,

J. NORMAN LOCKYER.

The Secretary of the Royal Society.

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These results were announced to the Paris Academy of Sciences by Mr. Warren De la Rue, in the following paper.

Sur une méthode employée par M. Lockyer pour observer en temps ordinaire le spectre des protubérances signalées dans les éclipses totales de Soleil.

"J'ai eu le plaisir de communiquer à M. Delaunay deux Lettres relatives à une découverte d'un de mes amis au sujet des protubérances roses qui se voient pendant les éclipses totales du Soleil. M. J. Norman Lockyer, en se servant d'un spectroscope construit exprès, a pu observer les lignes brillantes d'une protubérance superposées sur le spectre ordinaire, quand, en parcourant le bord du Soleil, l'instrument se trouvait sur un tel objet. Cette découverte a été faite le 20 de ce mois."

Première Lettre.—M. Balfour Stewart à M. W. De La Rue.

"21 octobre 1868.

Mr. Balfour Stewart's letter.

"Lockyer a eu un triomphe; il a trouvé les flammes rouges avec son nouveau spectroscope. Il dit, 20 octobre :

"J'ai saisi une protubérance aujourd'hui avec le nouveau spectroscope et obtenu les positions de trois raies :

"Une = C exactement,

"Une = F à peu près,

"Une, de 8 ou 9 degrés de l'échelle de Kirchhoff, plus réfrangible que la raie D."

Deuxième Lettre.—M. J. N. Lockyer à M. W. De La Rue.

"Londres, 23 octobre 1868.

My second letter to Mr. De la Rue.

"Beaucoup de remercîments pour votre bienveillante lettre et vos félicitations; elles sont une récompense bien suffisante pour beaucoup de travail et de patience. Je dis cela parce que, pendant deux ans et demi j'ai travaillé presque contre toute espérance.

"Je m'étais contenté pour le moment d'avoir envoyé une Note au Dr. Sharpey (Secrétaire de la Société Royale), et d'avoir annoncé mon observation à M. Stewart, qui, par hasard, passa chez moi le lendemain du jour où je l'ai faite; car je désirais compléter la chose, ou au moins lui donner quelque extension avant d'en parler davantage.

"Néanmoins je ne puis que vous remercier de ce que vous avez fait : la communication à l'Académie par votre entremise aura pour moi une très-grande valeur.

"J'ai été assez heureux pour trouver encore la protubérance hier matin (22 octobre), et j'ai confirmé tout : des raies (brillantes) en C, près de D et très-près de F. Certainement il n'y avait aucune raie en B ni en *b*. Quant à la région G, je ne l'ai pas encore examinée.

"J'avais lu la Note de M. Rayet, dans la nuit qui a précédé mon observation de la protubérance. Je crus d'abord que je n'avais observé que trois raies, sur neuf dont l'observation était possible. Mais en regardant de nouveau le diagramme de M. Rayet, dans les *Comptes rendus*, je crois que ce diagramme éclaircit l'affaire à un certain degré.

Lines observed.

Vous verrez qu'*il donne une longueur plus grande à trois raies* ; je pense que toutes les autres raies qu'il a vues viennent de cette partie très-brillante du spectre solaire ordinaire, que l'on voit quand on observe la région juste au delà du bord du Soleil. Il me semble que cette explication est d'autant plus probable que M. Rayet a observé avec une fente très-large. Du reste, il existe dans le spectre solaire une région extrêmement brillante, entre les deux raies les plus réfrangibles de *b*, exactement dans la position où M. Rayet place une raie courte ; il existe aussi une partie très-brillante entre *b* et F où il place une autre raie.

" En résumé :

" 1° J'ai déterminé trois raies :

" 2° Rayet donne trois raies plus longues que les autres ;

" 3° Tennant est sûr de trois raies ;

" 4° Herschel est sûr de trois raies.

" Il me semble que les lettres de Herschel et de Tennant et aussi le diagramme de Rayet font voir que leur nomenclature repose essentiellement sur une estimation plus ou moins rigoureuse, et non sur des mesures ; aucun de ces Messieurs ne paraît avoir pensé à mettre dans le champ de son télescope une échelle faiblement illuminée.

" Ainsi mes trois raies peuvent, après tout, représenter une plus grande portion du travail accompli que je ne l'avais d'abord imaginé.

" Avec une fente étroite, les raies ont été vues jusqu'à une petite distance *sur la surface même du Soleil.* C est de beaucoup la raie la plus brillante, et Mme. Lockyer a pu l'apercevoir sans difficulté.

" Les raies se prolongeaient à des hauteurs différentes au delà des bords du Soleil ; la rouge était la plus courte. J'ai même pu me rendre compte de la forme de la protubérance qui a dû être celle qu'indique cette figure :—

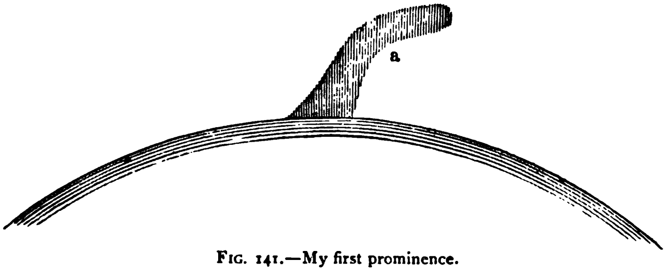

FIG. 141.—My first prominence.

" Quand la fente a été ajustée de manière à tomber sur *a*, la raie brillante s'est trouvée entièrement séparée du spectre solaire.

" Voici un autre fait : quoique C et F soient considérées toutes deux comme étant les raies de l'hydrogène, elles n'avaient pas cependant des longueurs égales : la rouge s'approchait davantage du Soleil."

The reading of this letter was followed by a communication from M. Janssen, received the same day. I cannot

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refrain from printing this, and M. Faye's remarks on both the communications, in this place :—

Indication de quelques-uns des résultats obtenus à Guntoor, pendatn l' éclipse du mois d' août dernier, et à la suite de cette éclipse. Lettre de M. Janssen à M. le Secrétaire Perpétuel.

"Cocanada, 19 septembre 1868.

M. Janssen's letter.

"J'arrive en ce moment de Guntoor, ma station d'observation de l'éclipse, et je profite à la hâte du départ du courrier pour donner à l'Académie des nouvelles de la mission qu'elle m'a fait l'honneur de me confier.

"Le temps me manque pour envoyer une relation détaillée ; j'aurai l'honneur de la faire par le prochain courrier. Aujourd'hui je résumerai seulement les principaux résultats obtenus.

"La station de Guntoor, a été sans doute la plus favorisée : le ciel a été beau, surtout pendant la totalité, et mes puissantes lunettes de près de 3 mètres de foyer m'ont permis de suivre l'étude analytique de tous les phénomènes de l'éclipse.

"Immédiatement après la totalité, deux magnifiques protubérances ont apparu ; l'une d'elles, de plus de 3 minutes de hauteur, brillait d'une splendeur qu'il est difficile d'imaginer. L'analyse de sa lumière m'a immédiatement montré qu'elle était formée par une immense colonne gazeuse incandescente, principalement composée de gaz hydrogène.

"L'analyse des régions circumsolaires, où M. Kirchhoff place l'atmosphère solaire, n'a pas donné des résultats conformes à la théorie formulée par ce physicien illustre ; ces résultats me paraissent devoir conduire à la connaissance de la véritable constitution du spectre solaire.

"Mais le résultat le plus important de ces observations est la découverte d'une méthode, dont le principe fut conçu pendant l'éclipse même, et qui permet l'étude des protubérances et des régions circumsolaires en tout temps, sans qu'il soit nécessaire de recourir à l'interposition d'un corps opaque devant le disque du Soleil. Cette méthode est fondée sur les propriétés spectrales de la lumière des protubérances, lumière qui se résout en un petit nombre de faisceaux très-lumineux, correspondant à des raies obscures du spectre solaire.

"Dès le lendemain de l'éclipse, la méthode fut appliquée avec succès, et j'ai pu assister aux phénomènes présentés par une nouvelle éclipse qui a duré toute la journée. Les protubérances de la veille étaient profondément modifiées. Il restait à peine quelques traces de la grande protubérance et la distribution de la matière gazeuse était toute autre.

"Depuis ce jour, jusqu'au 4 septembre, j'ai constamment étudié le Soleil à ce point de vue. J'ai dressé des cartes des protubérances, qui montrent avec quelle rapidité (souvent en quelques minutes) ces immenses masses gazeuses se déforment et se déplacent. Enfin, pendant cette période, qui a été comme une éclipse de dix-sept jours, j'ai recueilli un grand nombre de faits, qui s'offraient comme d'eux-mêmes, sur la constitution physique du Soleil.

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"Je suis heureux d'offrir ces résultats à l'Académie et au Bureau des Longitudes, pour répondre à la confiance qui m'a été témoignée et à l'honneur qu'on m'a fait en me confiant cette importante mission."

M. Faye's remarks were as follows:—

M. Faye's remarks.

"Je demande la permission d'ajouter quelques mots à l'exposé si lucide de M. le Président, pour expliquer la singulière coïncidence des deux communications qui viennent d'être faites à l'Académie.

"Il est certain que l'idée première de la méthode par laquelle M. Janssen d'abord, puis M. Norman Lockyer, sont parvenus, l'un aux Indes le 19 août, l'autre en Angleterre le 20 octobre, à saisir par l'analyse spectrale et à mesurer des phénomènes invisibles jusqu'ici, a été imaginée et proposée en premier lieu par M. Lockyer, mais elle n'avait conduit à aucun résultat. Tout ce qu'on pouvait conclure des premières tentatives faites dans cette voie nouvelle était une négation, quant à la nature gazeuse des protubérances. Or cette conclusion, *à priori* peu admissible, a dû jeter quelque défaveur sur la méthode elle-même. Voilà ce qui m'explique le peu d'attention que les observateurs de l'éclipse ont donné à cette méthode, publiée depuis deux ans dans les *Proceedings de la Société Royale* de Londres. Les astronomes anglais eux-mêmes ont négligé d'en faire l'application dans leur expédition des Indes à la suite de l'éclipse du 18 août.

"L'insuccès des tentatives premières de M. Norman Lockyer (il est aisé de s'en rendre compte aujourd'hui) me paraît tenir à ce que ce savant, dans l'impossibilité où il était alors de prévoir de quelles raies lumineuses se composerait le spectre des protubérances supposées gazeuses, ne savait sur quelles particularités délicates du spectre si compliqué des régions circumsolaires il devait porter son attention. Cela est si vrai, que c'est seulement quand il a su, par les observateurs français et anglais de l'éclipse, la nature détaillée du spectre des protubérances, qu'il a réussi à trouver en Angleterre les traces de ce spectre dans celui des régions voisines du bord du Soleil.[1]

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[1] L'idée de la méthode a été exposée pour la première fois dans un Mémoire communiqué à la Société Royale, le 11 octobre 1866, sous le titre: "Spectroscopic Observations of the Sun, by Norman Lockyer." L'objet principal de ce Mémoire était l'étude du spectre des taches, mais l'auteur finit par ces mots: "and may not the spectroscope afford us evidence of the existence of the red flames which total eclipses have revealed to us in the sun's atmosphere, although they escape all other methods of observation at other times? and if so, may we not learn something from this of the recent outburst of the star in Corona?" L'auteur ne s'est pas contenté d'indiquer cette méthode: il l'a appliqué avec persévérance pendant deux ans à la recherche des "red flames." Malheureusement il aura sans doute perdu de vue l'analogie qu'il avait si bien signalé lui-même, entre le spectre de l'étoile merveilleuse de la Couronne et le spectre probable des régions circumsolaires occupées par des protubérances, analogie devenue si frappante depuis la dernière éclipse. Aussi avait-il été forcé de conclure, par l'insuccès de ses premières tentatives, à la non-gazéité des protubérances. M. N. Lockyer a même reproduit tout récemment cette conclusion en juillet dernier, dans un article que

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M. Fayé's remarks.

"Nous ne saurions regretter que l'éloignement de M. Janssen ait laissé à M. Lockyer le temps de compléter, d'une manière indépendante, la découverte qu'il avait médité et si longtemps poursuivi. M. Janssen, de son côté, se trouvait en face du phénomène révélateur de l'éclipse ; il a su l'interpréter aussitôt ; il a réussi, en véritable maître lui aussi, et le premier, à découvrir ce qu'on avait cherché longtemps, mais infructueusement avant lui. Si donc la priorité de l'idée appartient sans conteste à M. Norman Lockyer, celle de la réussite et de l'application féconde revient de droit à M. Janssen, car, sans la distance, nous aurions eu deux mois plus tôt, et par lui seul, ces révélations merveilleuses sur les régions circumsolaires.

"Mais au lieu de chercher à partager, et par conséquent à affaiblir le mérite de la découverte, ne vaut-il pas mieux en attribuer indistinctement l'honneur entier à ces deux hommes de science qui ont eu séparément à plusieurs milliers de lieues de distance, le bonheur l'aborder l'intangible et l'invisible par la voie la plus étonnante peut-être que le génie de l'observation ait jamais conçue ?"

Supplementary Note on a Spectrum of a Solar Prominence.

By J. NORMAN LOCKYER, F.R.A.S., in a letter to the Secretary. Communicated by Dr. SHARPEY, Sec. R.S. Received November 5th, 1868.

The continuous envelope.

SIR,—I have the honour, in continuation of my letter of the 20th ultimo, to inform you that I have this morning obtained evidence that the solar prominences are merely the expansion, in certain regions, of an envelope which surrounds the sun on all sides. I may add that other facts observed seem to point out that we may shortly be in a position to determine the temperature of these circumsolar regions.

I have, &c.,

J. NORMAN LOCKYER.

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j'ai longuement discuté sous d'autres rapports devant l'Académie (séance du 27 juillet 1868) : "In the first place a diligent spectroscope sweeping round the edge of the sun has not revealed any bright lines. This is strong negative evidence that they are not masses of incandescent vapour or gas ; for as the light from such vapour or gas is almost monochromatic, it should be as easy to detect as that of the immeasureably distant nebulæ."—*Macmillan's Magazine*, July 1868, p. 254, and *ante*, p. 77. See also Note B.

Complete Account.

Spectroscopic Observations of the Sun, No. II.—By J. N. Lockyer. Communicated by Dr. SHARPEY, Sec. R.S. Received November 19, read November 19 and 26, 1868.[1]

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Preliminary Remarks.

In my first paper under the above title, kindly communicated by Dr. Sharpey to the Royal Society in 1866,[2] was contained an account of the determination of the nature of sun-spots, by means of the spectroscope. The paper concluded as follows:—

"May not the spectroscope afford us evidence of the existence of the 'red-flames' which total eclipses have revealed to us in the sun's atmosphere, although they escape all other methods of observation at other times? and if so, may we not learn something from this of the recent outburst of the star in Corona?"

Before the paper was written I had diligently swept round the solar disc in search of evidence of the red flames, but without result. This want of success I attributed to the excessive brilliancy of the spectrum of the circumsolar regions in the field of view of the instrument employed. I found in fact (although I did not discontinue my efforts) that both for these observations and for those on sun-spots more dispersion was necessary; in one case to weaken the atmospheric light, in the other to widen the spectrum.

Aid sought from Government Grant.

I therefore represented my requirements to the Government-Grant Committee, and was at once supplied with funds to procure a spectroscope of the requisite dispersive power.

The construction of this instrument was commenced in the beginning of 1867 by Mr. Cooke, on a plan which had been arranged between us, but unfortunately it was

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[1] *Phil. Trans.* 1869, p. 425.

[2] *Proceedings of the Royal Society*, vol. xv. p. 256.

Cause of the delay in the preparation of the spectroscope.

never finished. Mr. Cooke's health was then already failing, and at last, at the end of the year, he begged me to consider the order cancelled. Under these circumstances, at the beginning of the present year I sought the assistance of Mr. Browning; but the construction was further delayed, partly on account of an illness which necessitated my absence from England. At last the instrument, which reflects great credit on Mr. Browning's skill, arrived on the 16th of October, 1868, not quite complete, but in a condition which enabled me to commence work.

I mention these facts, first to account for my apparent inaction, and secondly in order that the coincidence in time of my results with those obtained by the observers of the recent eclipse may not be misinterpreted.[1]

I began my work with the new instrument by continuing my search after the prominences. I found that the circumsolar light was now so greatly reduced that although the lines were faintly seen on a dimly coloured background, the background itself was apparently dark enough to render a bright line distinctly visible. My first attempts, however, with the new instrument, not yet in

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[1] October 11, 1869.—It is important that I should be allowed further to emphasize this remark, for M. Faye, who was unaware of the date on which my new spectroscope was received, has stated (*Comptes Rendus*, t. lxvii. (1868), p. 840), "L'insuccès des tentatives premières de M. Norman Lockyer (il est aisé de s'en rendre compte aujourd'hui) me paraît tenir à ce que ce savant, dans l'impossibilité où il était alors de prévoir de quelles raies lumineuses se composerait le spectre des protubérances supposées gazeuses, ne savait sur quelles particularités délicates du spectre si compliqué des régions circumsolaires il devait porter son attention. *Cela est si vrai, que c'est seulement quand il a su, par les observateurs français et anglais de l'éclipse, la nature détaillée du spectre des protubérances*, qu'il a réussi à trouver en Angleterre les traces de ce spectre dans celui des régions voisines du bord du soleil." I think that the illustrious French astronomer, who has otherwise done me such ample justice, will not object to my pointing out this slight inaccuracy, due entirely to the fact that the first communication of my discovery was incomplete in its statement of the circumstances which attended it. The bright lines as seen in my instrument are so obvious and brilliant that a child could not overlook them.—J. N. L., October 10, 1869. See also Note B.

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adjustment, were as unsuccessful as those made with the smaller one; and it was not till the 20th of October that, after sweeping for about an hour round the limb and arriving at the vertex of the image, near the south pole of the sun, I saw a bright line flash into the field.

The chromospheric lines first seen on the 20th of October 1868.

My eye was so fatigued at the time that I at first doubted its evidence, although, unconsciously, I exclaimed "At last!" The line, however, remained—an exquisitely coloured line absolutely coincident with the line C of the solar spectrum, and, as I saw it, a prolongation of that line. Leaving the telescope to be driven by the clock, I quitted the observatory to fetch my wife to endorse my observation.

Detail of the Observations.

Search for new lines.

October 20.—Having settled that the new line was absolutely coincident with C, I commenced to search for more lines. This I found very difficult, as the instrument requires several movements and adjustments for the various parts of the spectrum, and the rate of the driving-clock was not properly adjusted for the sun's motion; the prominence was therefore lost at times; moreover the observations were impeded by clouds.

I commenced the search for lines from C to A. B was first brought into the field with the newly discovered line at C. There were no new lines visible. I then made an excursion to A with no result, and returned to C to assure myself that the prominence was still on the slit.

Results.

I then worked from the line at C towards D. A little beyond D, the lines of which are widely separated in my instrument, I detected another single and less vivid line, by estimation 8° or 9° of Kirchhoff's scale more refrangible than the more refrangible of the strongest D lines. I could detect no line corresponding to it in the solar spectrum, but the definition was not good.

b was next tried, the excursion now being made from the new line near D. There was no line at *b*, though the new D line was still visible when I returned to it.

Appearance of the F *line in the prominence.*

In the same manner, after many interruptions from clouds, I tried F; here I found another line. As at first caught it was very long; and by moving the telescope very slightly backwards and forwards in right ascension, in one direction the line shortened and brightened, and was visible on the solar spectrum for some distance, in another direction it became disconnected with the spectrum altogether. I was hence able to determine roughly the shape and dimensions of the prominence.

Position of line at F *doubtful.*

It was extremely difficult to fix the exact position of the line at F, although I had had no difficulty or even cause for hesitation about the others. It seemed at times to lie athwart the F line in the faint spectrum, although at first it had appeared more refrangible, especially when it was visible on the solar spectrum itself.

October 22.—Two days afterwards, I had another opportunity of observing the prominence spectrum, and of endorsing everything I had observed on the former occasion. With regard to the F line, I traced the bright line parallel to the dark line and outside it (*i. e.* more refrangible) further than I did on the 20th, but once or twice I caught it inside F on the sun.

I found that a narrow slit was best for D and F, as there are bright bands in the solar spectrum at those points. The lines were always visible with the usual width of slit: C was bright enough to allow the slit to be widened.

Bright lines in Solar Spectrum.

On this day, as on the 20th, my attention was strongly drawn to certain bright regions in the spectrum.

October 27.—On this date the prominence first observed was no longer on the limb, I had to search therefore for a new one; I found one on the eastern equatorial limb. The method of observation previously adopted was changed, and a mixed spectrum of the sun's limb and prominence was obtained. The two spectra were in fact superposed.

In the case of the line at C, the result was to absolutely eclipse the dark line in the spectrum, and to replace it by a vividly bright band (Fig. 89). The behaviour of the F line was still a puzzle to me. In the spectrum of the light proceeding from the exact limb of the sun the bright line was seen more refrangible than F, (Fig. 100,) but in the spectrum of the prominence at some distance above the sun the black line F was eclipsed. This experiment, which I repeated several times, seemed in a measure to explain what I had before observed; and under this date I entered in my note-book, "It appears that away from the sun's surface the substance gives out less refrangible light than it does when apparently at the surface."

Remarkable behaviour of the F line.

November 5.—The next observations were made on this date under superb atmospheric conditions, and after an important alteration had been made in the instrument, enabling me to make the several adjustments with the utmost nicety.

After the adjustments to the sun's limb had been made, I at once saw what I imagined to be the indication of a small prominence, and swept for a development of it, thinking that the portion observed might be one of the loops or lower levels which generally separate the higher peaks. Having swept for some distance on both sides the region on which the telescope was clamped in the first instance, and finding everywhere the same uniformity of height, it at once struck me that I was in presence of something new, and that possibly what I was seeing might indicate a solar envelope. I rapidly, therefore, tried several other parts of the limb to test the idea. It was soon established. *In every solar latitude both the* C *and* F *bright lines were seen extending above the solar spectrum.* The spectrum near D was so bright that I was compelled to refrain from examining it, but I caught the line near D once. The thickness of this envelope I found to be sensibly uniform, except in the regions where it was heaped up with prominences.

The C and F lines are visible in every solar latitude.

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The F line widens as the sun is approached.

The spectrum of the envelope cleared up all the difficulties connected with the F line. Perfect definition and adjustment now enabled me to see that the base of the bright line widens out as the solar spectrum is approached, and that whereas the line, away from the sun, corresponds, in the case of an ordinary prominence, in refrangibility and thickness to the Fraunhofer line F, close to the sun it widens out, so as to overlap the F line on both its sides to an extent about equal to its thickness, so that it is three times broader, or perhaps more, on and close to the limb.

In the spectrum of a prominence in which violent action was going on the line thickened out in the same manner at some distance above the limb (Fig. 91). There was no thickening observed in the C line at the base, or in the case of the phenomenon just referred to.

I obtained on this day the outlines and dimensions of two prominences.

November 6.—The observations made on the preceding day were confirmed, and one of the prominences reobserved. I also suspected a new line a little less refrangible than C.

A companion to the C line detected.

November 8.—The line suspected on the 6th was seen several times to-day. It came out beautifully with very varying brilliancy and even colour, to judge by appearance and impressions. It is very much thinner than the bright line C, and seems only to flash out where great action is going on, which slightly thickens the C line at its base. It is so near the C line that when both are shining brilliantly the line looks a double one, like D in a spectroscope of moderate dispersive power.

Bright lines seen in the solar spectrum.

To-day for the first time I got definite evidence of what I had suspected on several occasions with regard to different parts of the spectrum. A bright line was observed in the solar spectrum itself, a little less refrangible than the line C.

On the Spectrum of the Prominences.

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The existence of three lines in the spectrum of the prominences and their approximate positions were determined and communicated to the Royal Society on the 20th of October.

The coincidence of the chromospheric lines with C and F determined

The coincidence of one of the lines with the solar line C was at once determined.

The coincidence of another line with F at a certain distance from the sun's surface was finally determined on the 5th of November, when the fact of the widening out of the lines towards the sun was discovered.

The exact position of D_3 determined.

The exact position of the line near D is shown in Fig. 87, in which it is laid down from the mean of three careful micrometrical measurements made under far from good atmospheric conditions on the 15th of November. In Kirchhoff's map the new line falls in a region where no line was measured by him. I may also add that, by the kindness of Mr. Gassiot, I have been enabled to inspect the very elaborate maps of the spectrum constructed at Kew Observatory. The measures above given make the new line fall between two lines of almost inconceivable faintness; in Mr. Gassiot's map, indeed, there are none but such lines for some distance on either side of the region in which the new one falls.

The existence of the line near C established.

On the 8th of November the existence of another line was definitely established; its position in the spectrum is slightly less refrangible than Fraunhofer's C.

It is very variable.

Unlike the other lines, which are seen in all prominences, this line is only visible at times, being rendered so apparently by the presence of certain conditions which are not permanent. Intense action going on in a prominence will sometimes render this line visible: I am not, however, prepared to say that this is always the case. The line when visible is much more variable than the others: at times it is the mere ghost of a line, at others it rivals the C line in brilliancy.

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C *more brilliant than* F.

Of the three lines C is generally more brilliant than F; but I should add that it is difficult to determine the relative brilliancy of the lines because they are never seen together in the field of view of my instrument. The relative brilliancy of the line, near D, I am not sure about, because its situation in the brightest portion of the spectrum not only renders comparison difficult, but renders any conclusion which may be formed little worthy of confidence. My observations so far (November 16th) induce me to ascribe great variability, not only to the absolute, but to the relative, brightness of the lines. One instance is of the utmost importance. On the 5th of November, in sweeping round the sun with the F line in the field of view, I came across a prominence in which action of the intensest kind was evidently going on; the light and colour of the F line were most vivid, the luminosity of the line was greater than that of any part of the solar spectrum then visible in the field (Fig. 91). The action was not general, but limited to certain points, as if the prominences were built up of clouds, and the action was intensest at their centres; where the light was most vivid the spectrum widened out to almost the same extent as at the base.[1] The C line exhibited all the variations of brightness, but they were unaccompanied by any very decided widening of the line. The spectrum was too bright to observe the effect on the line near D.

On the Dimensions and Forms of the Prominences.

The prominence first observed on October 20th on the (true) southern limb of the sun was an exceedingly fine one; its shape, as determined by sweeping the slit over it was as represented in Fig. 141: its apparent height was about 3′. I did not stop to measure it more accurately. This prominence was not so visible on the next day of observation (the 22nd), as the sun's rotation had carried it on to the disc.

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[1] This might have been an effect of irradiation, but I can scarcely think so.

The dimensions of some of the prominences measured.

On the 5th of November I obtained the outline of two brilliant prominences, one near the southern, the other near the northern limb of the sun. The extreme (measured) apparent height of one was 35,000 miles, of the other somewhat less; the former I estimated to extend along the sun for about 200,000 miles. The shape of the southern prominence had changed considerably by the next day, the bright peak being quite gone; at the same time the length of the main portion had extended as if the peak had been absorbed into it.

The prominences change from day to day.

The prominences therefore change from day to day; at present I have not measured any more rapid change, but these observations are of so delicate a nature that it is easy to imagine rapid changes to be going on in any prominence of decided outline; for an error in the adjustment of the instrument with regard to the meridian or latitude, the least variation in the rate of the driving-clock, or any oscillation of the telescope-tube or the spectroscope, brings the slit on another part of the outline of the prominence, and under these circumstances the length of the line is perpetually varying.

It must be borne in mind that the dimensions of the prominences cannot be determined absolutely, as we do not know whether they are actually on the sun's limb at the time of measurement. Measurement can only fix a minimum.

On the Continuous Solar Envelopes.

The continuity of the envelope is established; it is named the Chromosphere.

The continuity of this envelope, which I propose to name the chromosphere, a name suggested by Dr. Sharpey, was spectroscopically established on the 5th of November, and an account of the observations was transmitted to the Royal Society on the same day.

By careful estimations made on the 6th of November (which are estimations only, for I had not yet mounted

The Chromosphere is about 5000 *miles in thickness.*

a wire-micrometer on the spectroscope-telescope), its general thickness was determined to be about 5000 miles; the level of its upper surface was not absolutely uniform in all latitudes, but it was very nearly so. I could detect no difference in the general level as between the equatorial and solar regions of the sun.

It would appear that the light by which its existence is revealed proceeds from the same substance or substances of which the prominences are composed; and I hold the prominences to be merely the heaping together of the new envelope in some localities.

Under proper instrumental conditions the spectrum of this envelope can always be seen whenever the sun is shining. The spectrum consists of a line coincident with Fraunhofer's C, another more refrangible than D, and another coincident in the main with F. I say coincident in the main, because when the spectrum of the envelope is viewed so that it appears to rest on the solar spectrum, the line at F takes the form of an arrow-head (Fig. 88), the shaft of the arrow being represented by the black line F in the solar spectrum itself. This results from a thickening out of the line as it approaches the sun. At the outer surface of the envelope the refrangibility of the light is the same as that of the line F; but, as the sun's surface is approached, the line widens gradually into a band overlapping F by more than its own thickness on either side, and more on the more refrangible side than on the other. The black line F also loses its thickness as it approaches the boundary of the spectrum. The amount of widening at the base is subject to variation.

The F *line becomes thick as it approaches the limb.*

C *and* D *do not become thick.*

In the spectrum of the chromosphere this widening at the base is not generally observed either in the line at C or in that near D.

None of the lines stop sharply; they all fade out as the limit of the envelope is approached.

On certain Bright Regions in the Solar Spectrum.

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From the commencement of my observations with the new instrument my attention has been drawn to certain bright regions in the ordinary spectrum; but it was not till the 8th of November, 1868, that I succeeded in observing a definite bright band extending for a certain distance on the sun near the limb.

The F *and* C *lines seen bright on the limb itself.*

I should state that I have observed this behaviour in the F band on either side of Fraunhofer's dark line F, and in the C line, when the prominence, as I have imagined, has extended from the limb over the earth's side of the sun.

The position of the bright band observed on the 8th of November is near C, but slightly less refrangible, not far from the place in the scale occupied by the last discovered red line, the position of which as yet has not been micrometrically determined.

Other regions to which my attention has been particularly drawn from the first, although up to the present time I have obtained no results, lie, one between the *b* lines, another between *b* and F, another less refrangible than B, one near D, and another near G.

Bright regions may be due to faculæ.

It is quite possible that these bright regions, the light of which is variable, may be due to faculæ; this conclusion is strengthened by the fact that diligent sweeping within the limb has not revealed the bright lines of the chromosphere spectrum. If this be so, the faculæ are not the prominences, although they may be possibly connected with them.

On the nature of the Chromosphere and Prominences.

It has already been concluded by M. Janssen, from the coincidence of two of the bright lines with C and F, that the prominences are composed of hydrogen.

So far as our present knowledge goes, however, this does not dispose of the other two bright lines, the positions of

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which have been determined by myself: I allude to the lines near D and near C.

At the present moment I am engaged on a series of experiments on gaseous spectra, which I hope will afford additional information on these points; in the interim, on the assumption that the chromosphere and prominences are wholly, or in part, composed of hydrogen, several considerations which appear to me of great importance may be touched upon.

These considerations are based upon the experiments of MM. Plücker and Hittorf[1] on the one hand, and of Professor Frankland on the other.[2] In MM. Plücker and Hittorf's paper entitled "On the Spectra of Ignited Gases and Vapours, with especial regard to the different Spectra of the same elementary Gaseous Substance," these investigators point out the effect of temperature on the different spectra, the temperature of the discharge of Ruhmkorff's induction-coil being increased by increasing the power of the inducing current, or, preferably, by diminishing the duration of the induced one, by means of the Leyden jar.

Effect of increase of temperature on the light emitted by gases.

Among the important results obtained in the case of nitrogen were the following. With increase of temperature the light passes through the following colours:—

Golden,
Bluish violet,
White,

and at an extreme temperature the lines expand, approaching thus to a continuous spectrum.

We now come to hydrogen. When a Geissler's tube filled with extremely rarefied hydrogen is used, the spectrum produced by a spark of low temperature consists of three lines of the same width as the slit—one red, another bluish green, another violet;[3] these are respectively termed

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[1] *Philosophical Transactions*, vol. clv. (1865), Part I. pp. 1—29.

[2] *Proc. Roy. Ins.*, vol. v. p. 419.

[3] I am aware that Ångström has discovered another line, but it is not necessary here to consider it.

Hα, Hβ, and Hγ by MM. Plücker and Hittorf. The places of these lines in the solar spectrum are at C, at F, and one at some distance from G towards F.

I quote the following results of increase of temperature from the memoir under notice :—

Plücker's and Hittorf's memoir.

"Hydrogen shows in the most striking way the expansion of its spectral lines, and their gradual transformation into a continuous spectrum. When the direct discharge of Ruhmkorff's large induction-coil is sent even through the old spectrum-tubes enclosing hydrogen, the formerly obtained spectrum is essentially altered. By increasing the power of the coil, the violet line Hγ first expands; while it continues to expand, the expansion of the bluish-green line Hβ becomes visible. Let the aperture of the slit be regulated so that the double sodium-line will separate into two single lines nearly touching one another. Then, the angular breadth of Hβ becoming two or three minutes, the breadth of Hγ is about double. The expansion takes place as well towards the less as towards the more refracted part of the spectrum. Hα remains almost unchanged after Hγ has passed into an undetermined large violet band, and Hβ extended its decreasing light on its two sides. On employing the Leyden jar, and giving to the gas in our new tubes a tension of about 60 millims., the spectrum is already transformed into a continuous one, with a red line at one of its extremities. At a tension of 360 millims. the continuous spectrum is highly increased in intensity, while the red line, Hα, expanded into a band, scarcely rises from it. If the electric spark passes through hydrogen at the ordinary tension, the ignited gas on its way always gives the spectrum of the three expanded lines.

New bright lines seen in the Spectrum of hydrogen.

"Even in the old spectral tubes inclosing highly rarefied hydrogen, the ground, from which the three characteristic lines rise, did not appear always of the same darkness; in some instances new bright lines appeared, *especially in the neighbourhood of the sodium line.* In resuming the subject, we pointed out the existence of a new hydrogen-spectrum, *Plücker and Hittorf's experiments on gaseous spectra.*

corresponding to a lower temperature, but having no resemblance at all to the spectra of the first order of nitrogen, sulphur, &c. In this spectrum, of a peculiar character, if fully developed, we observe a great number of well-defined bright lines, almost too numerous to count and represent by an engraving, but brilliant enough to be examined with a magnifying-power of 72, after the light has passed through four prisms.

"On sending the direct discharge of Ruhmkorff's coil through a tube of glass from one-fourth to one-eighth of an inch in diameter, provided with electrodes of platinum or of aluminium, inclosing hydrogen at a tension of 5 to 10 millims., a luminous thread of light of a bluish-white colour was seen passing along the axis of the tube, without touching the glass. When analysed by the prism, it gave a faint spectrum of the above-mentioned numerous bright lines, especially within the red and the yellow. Among these lines neither Hα nor Hγ were seen; Hβ only appeared, but less bright than many other lines. By interposing the Leyden jar and gradually increasing its charge, all the lines became brighter, Hβ surpassing all other lines in brilliancy; Hα appeared beautifully, Hγ fainter. Hence we conclude that the numerous bright lines belong neither to the vaporized metal of the electrodes, nor to the decomposed interior surface of the glass, but solely to the hydrogen, constituting a new spectrum of it. This spectrum may be seen simultaneously with the three characteristic lines Hα, Hβ, Hγ; but at an increased temperature, when these lines begin to expand, it entirely disappears.

MM. Plücker and Hittorf also determined the action of the discharge of a small induction-coil through a tube inclosing hydrogen, which was gradually rarefied to the highest tenuity to be reached by a Geissler's exhauster; they found that, as the exhaustion proceeded, the red colour of the gas gradually faded into an "undetermined violet," and that Hα disappeared, while Hβ, though fainter, remained well defined.

With hydrogen gas in Geissler's tubes, then, the following facts are established:—

I. With a certain degree of rarefaction and temperature, we obtain three characteristic lines, Hα, Hβ, Hγ.

Effects of pressure and temperature on the hydrogen spectrum.

II. By increasing the temperature, we expand Hγ first towards both ends of the spectrum, then Hβ, Hα remaining almost unchanged after Hγ has passed into an undetermined large violet band.

III. By increasing the tenuity, Hα disappears first, Hβ remaining well defined, and moreover the colour of the ignited gas changes to the eye.

IV. Under certain conditions, which are not stated in the memoir, new lines appear in the spectrum, especially in the neighbourhood of the sodium line.

Assuming that hydrogen gas is present in the chromosphere and prominences, we have the following facts to place side by side with those just stated:—

I. In place of the three lines we have but Hα and Hβ.

II. Hβ is in process of expansion, the expansion increasing as the sun is approached, and Hγ is so far expanded that it no longer exists as a line; most careful observations repeated several times have failed to detect it. Were it a broad band having the same total amount of light, it would be invisible in the spectroscope; it has probably therefore reached this stage.

III. The prominences have been observed of various colours (this fact is not here stated merely with reference to the observation recorded in III. *ante*).

The length of the D_3 line is the same as that of C and of F.

IV. *There is a line in the yellow*, most probably proceeding from the substance which gives off the light at C and F, as the length of this line, as far as the later observations with the more correctly adjusted instrument go, is the same as that of those in C and F.

I am aware that the conditions as to density cannot for one moment be held to be the same in the two cases; but as at present (so far as I know) we have no similar experiments ranging over greatly varying densities, I have thought it desirable to bring these striking facts forward at once. We are justified in thinking that the density of the chromosphere, always assuming that it is composed wholly or in part of hydrogen, cannot be very great; if it were, the spectrum would most probably be continuous; for Professor Frankland has shown, in the lecture before alluded to, that hydrogen burning in oxygen under a pressure of ten atmospheres gives out a spectrum, bright, and perfectly continuous from red to violet.

Frankland's experiments of the causes of luminosity in flame.

It is possible that experiments in which both density and temperature are varied may enable us to match accurately the spectrum of the chromosphere, and thereby determine both the temperature and pressure at the surface of the sun.

The bright lines which have been observed in several stars, especially in the remarkable one in Corona, the outburst of which was spectroscopically watched by Mr. Huggins in 1866, show us that under certain conditions of constitution a chromosphere may be a part of the regular star-economy, and be liable to changes more or less great, and effected with more or less rapidity. The continuous observation of the sun's chromosphere may therefore lead to a knowledge of many important facts.

It is remarkable that in the outburst in the star in Corona, two of the lines of the chromosphere were coincident with C and F, and that two other lines were visible. If, therefore, either or both of these lines be due to hydrogen, we must regard that star, leaving the density of the envelope out of consideration, as being of lower temperature than our sun.

The position of the prominences in all parts of the sun's limb has generally been held to be sufficient evidence that they are not connected with spots. They may, however, still result from some common cause. It may be noted that the eclipse which was photographed by Mr. De la Rue in Spain happened near the time of maximum sun-

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spots, and that nearly the whole limb of the sun was covered with prominences. My observations since 1866 have been carried on at the minimum sun-spot period, and the prominences observed during the eclipse this year were few in number, and covered but a small part of the sun's limb.

Additional Note on the Chromosphere.[1]

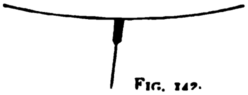
Since my last communication to the Royal Society I have received, through the kindness of Admiral Manners, the following extract of a note from Father Secchi, in which, although the existence of the new continuous envelope is not announced, important corroboration of its existence is contained. Father Secchi says:—[2]

Father Secchi's note.

"Si l'on met la fente parallel à la tangente du bord . . . . en arrivant plus près du bord la ligne devient continue. Cette observation prouve que la couche gazeuse rose est continue, mais très-irrégulière dans son contour, comme l'ont montré les éclipses."

"*Rome, November 15th*, 1868.

"I have been able to verify the observations of Mr. Lockyer on the sun, but I find that, even where the lines do not become brilliant, their blackness vanishes by a partial inversion. I have found also some luminous lines which become exceedingly brilliant near the edge of the sun. One is near the ray D, and the other in contact almost with the line B on the side of C.

"*I find that all around the full limb of the sun the inversion takes place.* If the slit is perpendicular to the edge the inversion is a very short part, so  FIG. 147. of 10″ or 15″, but if the slit is parallel to the edge then the inversion is complete. It is a very beautiful fact. It is perhaps one that will modify our ideas on the origin of these lines."

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[1] Received November 26, 1868.

[2] *Comptes Rendus*, vol. lxvii., p. 1018. Letter of the 13th November, printed 25th November, 1868.

I have italicized the most important part of the letter.

Examination of De la Rue's photographs.

Since the 20th of the present month, in consequence of a conversation on that day with Mr. De la Rue, I have gone over my observations of the sun's limb with great care, and have also re-examined Mr. De la Rue's photographs with a view to ascertain the evidence which they give of the continuity of the envelope. The result strongly confirms my former views. It is true that the photographs do not show a continuous chromosphere of anything like uniform thickness; but that arises from the fact that the only part of the sun's limb where the envelope was visible at all during totality happened to be covered by irregular prominences, which were probably very abundant at the time. In fact, owing to the relative sizes of the sun and moon during the eclipse of 1860, and the direction of the moon's motion the top and bottom of the sun's limb, as shown in plate 15 of Mr. De la Rue's memoir, were considerably within the moon's limb during the totality; only small portions of the sun's limb on the preceding and the following sides were uncovered, and there the chromosphere is seen to be almost continuous, though, as I have said, very irregular. The absolute want of continuity in the photographs, which, as far as I recollect, is not observable in Mr. De la Rue's hand drawing, is not a final argument against its real spectroscopic continuity. On the 24th of November 1868, when I had an opportunity of observing the sun, I employed the whole time in examining into the absolute continuity of the chromosphere; I was enabled, until interrupted by clouds, to examine continuously and carefully about 300 degrees of the sun's contour, and during the time I did not for one moment lose sight of the chromosphere spectrum. It is very gratifying to find that photographs taken many years ago by so excellent an observer as Mr. De la Rue should

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be found to bear independent testimony to the accuracy of the conclusion which I have arrived at, by an entirely different line of research.

Historical Notice of the growth of our knowledge of the Chromosphere.[1]

When I was first enabled, by means of the spectroscope, to determine that the prominences are merely heapings up of an envelope which gave everywhere the spectrum of hydrogen (at pressures which Dr. Frankland and myself have since approximately determined) and is continuous round the sun, or at all events continuous in the same sense that the photosphere is continuous, I was not aware that I had been anticipated in any part of the discovery.

The continuity of the envelope long suspected.

I have since found, however, that the continuity of the envelope, apart from its nature and place in the solar economy, has been suspected for many years, although it had never been demonstrated, as it easily might have been, by eclipse observations at properly chosen stations; and although it has been very variously interpreted.

I think it desirable, now that the spectroscope has determined the existence and nature of such an envelope, that, in justice to myself and to those who have gone over the same ground before me, a brief historical notice of the progress of our knowledge on this point should be given.

It is easy now to divide the phenomena observed in the many eclipses between 1706 and 1842 into two classes:

I. Observations of the prominences properly so called;

II. Observations of the chromosphere;

and all the observations of both these classes, accumulated during the eclipses which happened up to and including the year 1842, have been discussed by two eminent astronomical authorities,—I refer to Arago and Professor Grant, to both of whom, long even before the eclipse of 1851, it was perfectly obvious that the prominences were solar and not lunar phenomena.

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[1] Received April 9, 1869.

Arago[1] considered the prominences to be merely clouds floating in the sun's atmosphere—an atmosphere rendered evident to us by the corona, and to these clouds he ascribed the spots without a nucleus; to the corona also he attributed the darkening of the limb. He says:—

Arago's statements.

"Il faut admettre une enveloppe extérieure qui diminue (étient) moins la lumière qui vient du centre que les rayons qui viennent sur le long trajet du bord à l'œil. Cette enveloppe extérieure forme la couronne blanchâtre dans les éclipses totales du soleil."

It is not easy to reconcile *all* Arago's statements as to the nature of this atmosphere or envelope; but I shall return shortly to a later enunciation of them by himself, merely remarking here that there is nothing said about the "clouds" forming a continuous envelope round the sun.

Grant's statements.

Professor Grant,[2] who went over the same ground as Arago, and had Arago's results before him, was led to consider before the eclipse of 1851 that "The observations of solar eclipses would seem to indicate that above the luminiferous envelope there exists a stratum of nebulous matter which is visible only by means of reflected light. Various interesting questions present themselves for solution in connection with the admitted existence of such a *stratum*. In the first place, does this *third envelope* exercise an influence in the production of any of the other phenomena which have been disclosed by observations on the physical constitution of the sun?"

Then, referring to Sir J. Herschel's hypothesis of convection currents throwing up the then imagined non-luminous envelope below the photosphere, through the photosphere, he adds:—

"This view of the subject, while it carries with it considerable probability, obviates the necessity of introducing into the theory of the physical constitution of the sun the idea of a third envelope independent of the two others."

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[1] *Annuaire du Bureau des Longitudes*, 1846. MSS. quoted by Humboldt, "Cosmos," Sabine's Translation, vol. iv., Notes, p. 102.

[2] "History of Physical Astronomy," p. 400.

Swan's memoirs.

We next come to the eclipse of 1851, which was observed by Professor Swan, among others, who contributed to the Royal Society of Edinburgh three valuable memoirs on the eclipse, with special reference to the red prominences. Assuming that they existed in the solar atmosphere, and agreeing with the prescient remarks of Sir J. Herschel, that they must be "cloudy masses of the most excessive tenuity," he remarks:[1]—

The prominence matter is distributed all round the sun.

"Obviously the simplest view that can be taken of this phenomenon, is to regard the red fringe and the red prominences as of the same nature; and all the observations will then confirm the idea that the matter composing those objects *is distributed all round the sun*. It therefore seems probable that when we are furnished with observations of a tangential phase of the eclipse from stations on the north side of the moon's shadow, it will be found that a sierra appeared towards the sun's north point, of which the detached prominences seen in that region, by observers situated near the middle of the moon's shadow, were only the highest peaks . . . . . Since, then, it has been shown to be highly probable that the matter composing the red prominences is distributed with little interruption all round the sun: we may conceive the luminous strata of the solar atmosphere to be surmounted by an envelope of clouds of which the higher portions are visible beyond the moon's limb, at the central phase of a total eclipse, and which then constitute the red prominences. If [he continues, throwing out the suggestion previously made by Grant] it be thought that the hypothesis of two envelopes of cloud, one above and another below the luminous strata of the sun's atmosphere, introduces too great complication, we may avoid the objection by supposing that the envelope which occasions the penumbræ around the spots penetrates the luminous stratum, and exists, although in greatly different degrees of density, both above and below it.

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[1] *Transactions of the Royal Society of Edinburgh*, vol. xx., part iii., pp. 462, 463, 464.

The surface of the cloudy stratum is exceedingly irregular.

"If, then, we conceive that a stratum of cloudy matter surrounds the sun, of which the red prominences are the higher portions, the serrated appearance of the long range of prominences, seen by Mr. Dawes and Mr. Hind, sufficiently indicates that its general surface is exceedingly uneven, presenting the appearance of being covered with numerous eminences or ridges. But these irregularities are small when compared with the large hook-shaped prominence, and its companion the detached cloud, which were seen by most of the observers of the eclipse. . . . . Now, as the spots have been supposed to arise from upward currents causing apertures in the sun's luminous atmosphere, I conceive the higher red prominences, or those which remain visible at the middle of the total phase of a central eclipse, may in like manner be formed by the same, or similar, currents in the sun's atmosphere, breaking through the envelope of cloud that surrounds him, bending back the edges of the apertures they have formed, and sometimes carrying up detached masses of cloud, such as that which was seen at the late eclipse. We may, however, suppose the envelope of cloud to be sometimes simply raised, without being broken through; and in that state it may form the conical prominences which were observed at the late eclipse.

The absorptive envelope.

"Since the prominences reflect, they must also absorb light; and thus the hypothesis which has been proposed regarding them *assumes the presence of an envelope of cloud surrounding the sun's luminous atmosphere, capable of absorbing part of his light and subject to occasional interruptions of its continuity.*"

Mr. Swan thus formally sums up his most important addition to our knowledge of the solar envelopes, agreeing

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with Arago that the corona is also a solar appendage. We have now four envelopes :—

Swan's four strata.

I. The dark cloud below the photosphere. (The cloudy stratum of Herschel.)

II. The photosphere itself.

III. The envelope of cloud so often referred to.

IV. The sun's atmosphere surrounding all, in which the other envelopes may be supposed to float.

Littrow's opinion.

Littrow, who observed the same eclipse, after describing some of the observations, goes on to say :—

"Tout cela me fortifie dans l'opinion conçue déjà par mon observation seule, que ce bord rouge forme une couche environnant toute la photospherè du soleil, et gonflée çà et là en protubérances."[1]

Arago's criticism on Swan's hypothesis.

Arago in his 'Astronomie'[2] defends his first view, and criticises Mr. Swan's hypothesis. He remarks :—

"Tout s'explique dans l'hypothèse de nuages flottants dans l'atmosphère diaphane qui entoure le soleil.

"J'avais cherché de rendre compte des protubérances lumineuses en les assimilant à des nuages flottants dans l'atmosphère diaphane dont je supposais la photosphère entourée. M. Swan ayant sans doute remarqué dans ma notice cette phrase 'L'éclipse de 1842 nous a mis sur la trace d'un troisième enveloppe située au-dessus de la photosphère et formée de nuages obscurs ou faiblement lumineux,' accumule à la fin de son mémoire citations sur citations, pour prouver que nonobstant ce que cette phrase paraît de renfermer de positif, je n'ai pas eu la pensée qu'il existât au-dessus de la photosphère une couche continue de nuages. JE RECONNAIS LOYALEMENT QUE L'IDÉE DE LA COUCHE CONTINUE APPARTIENT EN PROPRE À M. SWAN."

The next criticism we find brings in the corona in a way we shall see it again brought forward subsequently.

"La limite extérieure *de la première couronne* lumineuse indiquerait, dans l'hypothèse de M. Swan, la région

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[1] *Astr. Nachrichten*, t. xxxiv., p. 31, and *Comptes Rendus*, Feb. 22, 1869.

[2] Ed. 1856, t. iii. p. 623.

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Arago's criticism continued.

qu'occupe la couche continue de nuages dont il croit avoir besoin pour expliquer tous les phenomènes des éclipses totales. Il faudrait donc supposer que, lorsque la couronne est unique, cette couche de nuages s'est abaissée jusqu'à être presque en contact avec la photosphère solaire. C'est alors qu'apparaîtraient les longs arcs courbés, colorés et fortement dentelés, qui ont été signalés par les observateurs somme étant visibles quelques instants après les commencement de l'éclipse totale, et quelques instants avant la fin. Mais admettant pour un moment que ces grands mouvements oscillatoires en hauteur de la couche nuageuse existent, pourquoi cette couche se présenterait-elle comme une ligne circulaire sans couleur lorsqu'elle serait à une grande hauteur, et descendrait-elle irisée et très-irrégulière dans son contour lorsqu'elle se rapprocherait du soleil. Suivant M. Swan, les protubérances sont des portions de son atmosphère continue, soulevées du-dessus du niveau général par le courant ascendant. Mais comment n'a-t-il pas remarqué qu'en 1842 ces protubérances existaient tout notablement au-dessous de la ligne circulaire qui dessinait les limites de la couronne la plus brillante sur la couronne extérieure ?

"Je persiste donc à soutenir simplement que la troisième atmosphère solaire que M. Swan veut bien admettre avec moi est gazeuse et qu'il y flottent seulement des nuages."

This strong criticism would seem to have had great weight, and I believe that much of the strange modern overlooking of the continuous envelope is to be ascribed to it, to say nothing of the invariable (so far as I know) descriptions of the solar atmosphere to be found in our text-books, on which recent ideas and theories have been based.

We next come to the eclipse of 1851, which was observed in the Brazils. The Brazilian Commission, of which M. Liais was a member, sent in a report to the Paris Academy of Sciences, which was reported on by M. Faye, who commenced his report by thus defining the corona and prominences:[1]—"La couronne lumineuse ne serait autre chose que l'indice sensible d'une troisième enveloppe du soleil, d'une atmosphère extérieure. . . . *Les protubérances seraient les nuages de cette troisième atmosphère.*"

Faye's report on Liais's observations.

I have not been able to obtain the Commissioner's report; but M. Liais, in a separate work,[2] states distinctly (I.) that there is a continuous envelope overlying the photosphere, (II.) that it is not the corona, (III.) that it is the locus of the general absorption of the photospheric light, and (IV.) that its height is about 3″·3—an immense step in advance, as we now know, on the ideas of Arago. At the same time M. Liais was convinced that the corona was in reality a solar appendage.

Liais is convinced that the corona is solar.

The famous Spanish eclipse of 1860 is next on the list. Mr. De la Rue's admirable photographs have made us all familiar with the solar appendages then visible. Specially to be noticed in them are the points where the limb of the sun and moon were nearly in contact both at the commencement and at the end of the totality.

In discussing the results of this eclipse, both M. Le Verrier and Father Secchi found themselves compelled to use the word "envelope" to explain all the phenomena observed.

Le Verrier's discussion of the eclipse of 1860.

M. Le Verrier,[3] after a preliminary discussion of the results of this eclipse, remarked, "Faut il croire que la surface entière de l'astre en (nuages rouges) est parsemée jusqu'à une faible hauteur comme elle est semée de facules, et que les nuages roses en sont des émanations commes les taches qui apparaissent sur la disque de l'astre."

And then, after a more complete discussion, he endorses the idea of the complete continuity of the envelope, and makes it at the same time not only the only solar atmosphere, but the origin of spots!

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[1] *Comptes Rendus*, vol. xlviii., p. 163.

[2] "L'espàce Celeste et la Nature Tropicale."

[3] Report quoted in *Comptes Rendus*, February 8, 1869, p. 316.

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Le Verrier's discussion.

After referring to Herschel's hypothesis, and remarking "à cette constitution si complexe on eût dû ajouter une troisième enveloppe formée de l'ensemble des nuages roses," he goes on, "or je crains que la plupart de ces enveloppes ne sont de pures fictions; que le soleil ne soit simplement un corps lumineux, en raison de sa haute température, et recouvert par une couche continue de la matière rose dont on connaît aujourd'hui l'existence. L'astre, ainsi privé d'un corps centrale, liquide ou solide, recouvert d'un atmosphère rentre dans la loi commune de la constitution des corps célestes. . . . . Il paraît clair qu'ils (les protubérances roses) émanent accidentellement d'une couche de matière qui recouvre toute la surface du soleil jusqu'à une hauteur de 8 à 10 secondes. . . ."

M. Le Verrier then proceeds to show, as we have seen Mr. Swan and M. Liais do many years before, that the darkening of the limb is due to this envelope, and then adds, "D'un autre côté il resulte de l'observation des nuages solaires que la matière de l'atmosphère s'accumule quelquefois en quantités plus considérables sur certains points, et comme la lumière de la partie correspondante du soleil peut se trouver plus ou moins éteinte, on arrive à une explication naturelle de l'existence des taches!"

We see, then, that the French results of the eclipse of 1860 entirely endorsed Mr. Swan's assertion of the continuity of the cloudy envelope, although the hypothesis put forward to explain the envelope was completely discordant with observation then and is much more so now.

Father Secchi's report.

Father Secchi's idea will be rendered evident by the following extract from his report contained in the memoirs of the Observatory of the Collegio Romano:[1]—

"La seconda conseguenza, non meno importante è, che questa materia riveste tutta la superficie solare come un generale inviluppo transparente. Infatti il loro numero prodigioso, e il loro estendersi per archi continuati di molti gradi, ci mostra che è irragionevole supporle, particolarità locali ed eccezionali sulla superficie solare, come sono le macchie, nè possono dirsi eruzioni vulcaniche di pochi punti: al contrario il vederle spuntare congiunte in lunghe catene tanto al principio che al fine della totalità, ci persuade che negli altri punti della circonferenza si rendono visibili solo le cime maggiori e più elevati, restando le minori e più basse coperte dal corpo lunare."

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[1] New Series, 1860-1862, vol. ii. No. 5, p. 43.

The continuity of the envelope not yet admitted.

Although these modern results would seem to have settled the question as far as it was possible to settle it by ordinary observations on the central line of totality, I cannot find that the idea of the continuity of the envelope was generally accepted in England or in France.

In 1861 we find M. Faye[1] calling upon observers of the eclipse that was to happen on the last day of that year to examine "si cette auréole (la partie de cette couronne la plus voisine du soleil) présente ou non le renversement du spectre solaire, c'est à dire si les raies obscures de Fraunhofer seront remplacées dans ce spectre par des raies brillantes." There is not one word about the "couche rose" of M. Le Verrier!

Sabine, Stewart, and Challis suggest that the red flames are solar auroræ.

After this in England we find General Sabine, Dr. Balfour Stewart, and Professor Challis independently arriving at the conclusion that the red flames are *solar auroræ*—a theory which I think plainly indicates that the idea that they formed part of a continuous envelope was not in their minds. Mr. Balfour Stewart, in a Lecture at the Royal Institution,[2] remarked:—

"In support of this hypothesis it may be remarked that, during the late total eclipse in Spain, Mr. De la Rue, by means of the Kew photoheliograph, proved that these red flames belong to the sun, and that they extended in one case to the distance of 70,000 miles beyond his photosphere. But, considering the gravity of the sun, we are naturally unwilling to suppose that there can be any considerable

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[1] *Comptes Rendus*, vol. lii., p. 679.

[2] *Proceedings of the Royal Institution*, vol. iv., p. 60.

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amount of atmosphere at such a distance from his surface; and we are therefore induced to seek for an explanation of these red flames amongst those phenomena which require the smallest possible amount of atmosphere for their manifestation. Now the experiments of Mr. Gassiot, and the observed height of the terrestrial aurora alike convince us that this meteor will answer our requirements best. And besides this, the curved appearance of these red flames, and their high actinic power, in virtue of which one of them, not visible to the eye, was photographed by Mr. De la Rue, are bonds of union between these and terrestrial auroræ."

Although Mr. De la Rue in his Memoir refers to the prominences being scattered widely over the sun's disc, it was not till December 1867 that the real meaning of the photographs in this particular was grasped. Mr. Stoney, in a paper communicated to the Royal Astronomical Society in that month, in which he refers to his hypothesis that the sun has an enormous atmosphere, which in eclipses projects far beyond the disc of the moon, in which atmosphere the vapours which give rise to the selective absorption of the photospheric light are situated at various heights according to their vapour-density, writes as follows:—

Mr. Stoney's paper.

"Directly outside the photosphere, there lies a stratum of the sun's atmosphere which is still hotter than the photosphere, and on the outer boundary of this hot region there appears to be a shell of excessively faint cloud, part of which is seen in Mr. De la Rue's photographs of the eclipse of 1860. It probably extends the whole way round the sun. It is therefore very desirable that this faint shell, which seems to lie at a distance of 8″ or 10″ from the edge of the sun's disc, should be observed, both from a central station and from stations close to the northern and southern limits of totality, so as to ascertain whether we have reason to presume it is continuous round the disc."[1]

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[1] *Monthly Notices, Royal Astronomical Society*, vol. xxviii., p. 19.

Broadly, then, up to the spectroscopic determination of its continuity and real nature, the story of the chromosphere is as follows:—

1842. Arago refers it to clouds at the base of the corona, and regards these clouds as the origin of non-nucleated spots, the dimming of the limb being due to the corona.

General results of the pre-spectroscopic observations and theories.

1842. Grant acknowledges the continuity of the envelope, ascribes its brilliancy to reflected light, and thinks it may be due to the "cloudy stratum" being driven by convection-currents through the photosphere.

1851. Swan terms it a new envelope of the sun, states that it shines by reflected light, and ascribes to it the dimming of the limb.

1851. Littrow describes it as a continuous thin envelope.

1858. Liais describes it as a continuous envelope, and gives its thickness as 3″·3.

1860. Le Verrier describes it as the unique atmosphere of a solid sun and the origin of all spots.

1860. Secchi acknowledges it as a third envelope.

1867. Stoney considers it as a stratum of cloud at a distance of 8″ or 10″ from the photosphere.

DESCRIPTION OF THE PLATES.[1]

Plate XXXVII.

Figs. 86, 87, and 88 show the position of the lines observed on October 20, 1868, and their usual form—*i.e.* the line F is broad at the base and gradually tapers upwards, while C, and the line near D (with no corresponding absorption-line ordinarily visible) do not as a rule, present this peculiarity with the instrument employed.

Fig. 91 shows the appearances of the F line observed on the 5th of November, 1868.

Figs. 89 and 100 show the appearances of the C and F

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[1] The Plates have been distributed throughout the book. See chapter xiv. Figs. 84, 85, 86, 87, 88, 89, 91, &c.

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lines observed with a tangential slit; in the case of F the bright line observed was sometimes on one side of the absorption-line, and sometimes put it out altogether (October 27).

Figs. 7, 8, and 9[1] are outlines of the prominences observed on the 20th of October and 5th and 6th of November 1868.

Plate XXXVIII.[2]

Description of spectroscope.

The spectroscope of large dispersive power, by means of which the work I have described in the former part of this paper has been done, is attached to a clock-work-driven refractor of 6¾ inches aperture and 98⅓ inches focal length, the definition of which is very fine, and worthy of the reputation of its makers, Messrs. Cooke and Sons, York.

Fig. 84 will give a general idea of the spectroscope and of its attachment to the large equatorial. In this figure are shown the eye-end of the telescope with finder and clamping and adjusting rods, and an internal tube which I have found it convenient to insert into the ordinary one to allow of the adjustment of the spectroscope to the direct solar image, and the variously enlarged images, without disturbing the spectroscope itself on its supports.

This internal tube carries the ordinary eyepiece-end on which the prismatic steel rods which support the train of prisms, collimator, and telescope are fixed and provided with means of rotation.

The figure also shows the arrangement of the instrument when the direct image is viewed. The eye-piece is taken out, and the image is allowed to fall on the slit-screen outside the telescope. A short collimator collects the light, which then passes through the seven prisms (of 45°) of dense flint glass, of which the specific gravity, refractive index, and dispersive power, as determined by Mr. Browning, are as follows :—

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[1] Fig. 7 only is given. See Fig. 141.

[2] Added October 10, 1869.

Specific gravity	3·91
Refractive index	1·665
Dispersive power	0·0752

Ordinarily the seven prisms are alone used, but when more dispersion is wanted I have found it very convenient to fix an extra prism of 60°, as shown in the figure; this makes the instrument for some part of the spectrum a direct-vision one; and I have further increased the dispersion by partly filling the small telescope with direct-vision prisms.

The adjustment of the spectroscope to the telescope allows of the slit being brought either tangentially or radially on any part of the sun's limb.

Slit for comparing spectra.

Fig. 143 shows a slit I have designed for comparing various portions of the solar surface with each other. The slit, for purposes of description, may be imagined to be divided into three portions. The central portion admits direct light from some part of the sun; the upper and lower portions admit light from two other parts of the sun reflected on the slit by means of combinations of prisms.

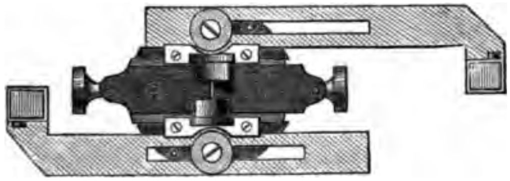

FIG. 143.—Arrangement for comparison of spectra of different parts of the sun.

The slit used in ordinary investigation is similar to that usually employed.

The train of prisms, the collimator, micrometer, and observing telescope are shown in Fig. 59 on a larger scale. The prisms are fixed to a plate which it is possible to rotate slightly on its axis, and this, after the more obvious precautions have been taken, constitutes one of the most important adjustments.

The collimator is furnished not only with the usual focussing-screw, but with additional rackwork, which permits of the slit being placed in the image, the colour of which is coincident with the arc under examination. It is only by paying infinite attention to this point that any good results can be obtained; and when this is done, and the atmosphere is pure and calm, the interval between each of the higher cloud-domes on the sun shows an approach to the spot-spectrum, and the spectrum is a mass of horizontal lines. It is rare, however, that the atmosphere is steady enough to show this effect in its greatest perfection.

The principal point about the micrometer-arrangement is, that the micrometer-head is read by a little telescope, the eye-end of which is immediately above that of the observing telescope; this saves much time, as the position of the observer is not disturbed when readings are necessary. I have also found it convenient to supplement this arrangement by an ordinary eyepiece-micrometer for differential measures of small range.

I cannot allow myself to conclude this short description without testifying to the great perfection of the optical finish of the new spectroscope, which enables me to obtain a solar spectrum of the utmost purity. My thanks are due to Mr. Browning for coming to my aid as he did, and employing his well-known skill in the construction of an instrument at once so unique, portable, and powerful—a task accomplished at last with results most satisfactory to myself and creditable to him.

THIRD PAPER.

Spectroscopic Observations of the Sun. No III.—By J. NORMAN LOCKYER. From the Proceedings of the Royal Society, No. 110, 1869. Received March 4, 1869.[1]

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SINCE my second paper under the above title was communicated to the Royal Society, the weather has been unfavourable to observatory work to an almost unprecedented degree; and, as a consequence, the number of observations I have been enabled to make during the last four months is very much smaller than I had hoped it would be.

Fortunately, however, the time has not been wholly lost in consequence of the weather; for, by the kindness of Dr. Frankland, I have been able in the interim to familiarize myself at the Royal College of Chemistry with the spectra of gases and vapours under previously untried conditions, and, in addition to the results already communicated to the Royal Society by Dr. Frankland and myself, the experience I have gained at the College of Chemistry has guided me greatly in my observations at the telescope.

Discovery of the third hydrogen line in the chromosphere ($H\gamma$).

In my former paper it was stated that a diligent search after the known third line of hydrogen in the spectrum of the chromosphere had not met with success. When, however, Dr. Frankland and myself had determined that the pressure in the chromosphere even was small, and that the widening out of the hydrogen lines was due in the main, if not entirely, to pressure, I determined to seek for it again under better atmospheric conditions; and I succeeded after some failures. The position of this third line is at 2796 of Kirchhoff's scale. It is generally excessively faint, and much more care is required to see it than is necessary in the case of the other lines; the least haze in the sky puts it out altogether.

Hence, then, with the exception of the bright yellow line, the observed spectra of the prominences and of the chromosphere correspond exactly with the spectrum of hydrogen under different conditions of pressure—a fact not only important in itself, but as pointing to what may be hoped for in the future.

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[1] *Proc. R.S.*, vol. xvii. p. 350.

D_3 *could not be found in the ordinary solar spectrum.*

With regard to the yellow line which Dr. Frankland and myself have stated may possibly be due to the radiation of a great thickness of hydrogen, it became a matter of importance to determine whether, like the red and green lines (C and F), it could be seen extending on to the limb. I have not observed this: it has always in my instrument appeared as a very fine sharp line resting absolutely on the solar spectrum, and never encroaching on it.

Dr. Frankland and myself have pointed out that, although the chromosphere and the prominences give out the spectrum of hydrogen, it does not follow that they are composed merely of that substance: supposing others to be mixed up with hydrogen, we might presume that they would be indicated by their selective absorption near the sun's limb. In this case the spectrum of the limb would contain additional Fraunhofer lines. I have pursued this investigation to some extent, with, at present, negative results; but I find that special instrumental appliances are necessary to settle the question, and these are now being constructed.

If we assume, as already suggested by Dr. Frankland and myself, that no other extensive atmosphere besides the chromosphere overlies the photosphere, the darkening of the limb being due to the general absorption of the chromosphere, it will follow:—

I. That an additional selective absorption near the limb is extremely probable.[1]

II. That the hydrogen Fraunhofer lines indicating the absorption of the outer shell of the chromosphere will vary somewhat in thickness: this I find to be the case to a certain extent.

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[1] See Note C.

III. That it is not probable that the prominences will be visible on the sun's disc.

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Chromospheric darkening of limb.

In connection with the probable chromospheric darkening of the limb, an observation of a spot on February 20th is of importance. The spot observed was near the limb, and the absorption was much greater than anything I had seen before; so great, in fact, was the *general* absorption, that the several lines could only be distinguished with difficulty, except in the very brightest region. I ascribe this to the greater length of the absorbing medium in the spot itself in the line of sight, when the spot is observed near the limb, than when it is observed in the centre of the disc—another indication of the great general absorbing power of a comparatively thin layer, on rays passing through it obliquely.

The spot-spectrum.

I now come to the selective absorption in a spot. I have commenced a map of the spot-spectrum, which, however, will require some time to complete. In the interim, I may state that the result of my work up to the present time in this direction has been to add magnesium and barium to the material (sodium) to which I referred in my paper in 1866, No. I. of the present series; and I no longer regard a spot simply as a cavity, but as a place in which principally the vapours of sodium, barium and magnesium (owing to a downrush) occupy a lower position than they do ordinarily in the photosphere.

What a spot is.

I do not make this assertion merely on the strength of the lines observed to be thickest in the spot-spectrum, but also upon the following observations on the chromosphere made on the 21st and 28th ultimo.

On both these days the brilliancy of the F line taught me that something unusual was going on; so I swept along the spectrum to see if any materials were being injected into the chromosphere.

On the 21st I caught a trace of magnesium; but it was late in the day, and I was compelled to cease observing by houses hiding the sun.

Spectral lines.

On the 28th I was more fortunate. If anything, the evidences of intense action were stronger than on the 21st, and after one glance at the F line I turned at once to the magnesium lines. I saw them appearing short and faint at the base of the chromosphere. My work on the spots led me to imagine that I should find sodium-vapour associated with the magnesium; and on turning from *b* to D I found this to be the case. I afterwards reversed barium in the same way. The spectrum of the chromosphere seemed to be full of lines, and I do not think the three substances I have named accounted for all of them. The observation was one of excessive delicacy, as the lines were short and *very thin*. The prominence was a small one, about twice the usual height of the chromosphere; but the hydrogen lines towered high above those due to the newly injected materials. The lines of magnesium extended perhaps one-sixth of the height of the F line, barium a little less, and sodium least of all.

Metallic vapours observed in the chromosphere.

We have, then, the following facts:—

The new facts.

I. The lines of sodium, magnesium, and barium, when observed in a spot, are thicker than their usual Fraunhofer lines.

II. The lines of sodium, magnesium, and barium, when observed in the chromosphere, are thinner than their usual Fraunhofer lines.

A series of experiments bearing upon these observations is now in progress at the College of Chemistry,[1] and will form the subject of a communication from Dr. Frankland and myself. I may at once, however, remark that we have here additional evidence of a fact I asserted in 1865 on telescopic evidence—the fact, namely, that a spot is the seat of a downrush, a downrush to a region, as we now know, where the selective absorption of the upper strata is different from what it would be (and, indeed, is elsewhere) at a higher level.

Messrs. De la Rue, Stewart, and Loewy, who brought

[1] See the account of the Laboratory work further on.

forward the theory of a downrush about the same time as my observations were made in 1865, at once suggested as one advantage of this explanation that all the gradations of darkness, from the faculæ to the central umbra, are thus supposed to be due to the same cause, namely, the presence to a greater or less extent of a relatively cooler absorbing atmosphere. This I think is now spectroscopically established; we have, in fact, two causes for the darkening of a spot:—

Causes of the darkness of a spot.

I. The general absorption of the chromosphere, thicker here than elsewhere, as the spot is a cavity.

II. The greater selective absorption of the lower sodium, barium, magnesium stratum, the surface of its last layer being below the ordinary level.

Possible fluctuations of the plane of condensation.

Messrs. De la Rue, Stewart, and Loewy also suggested, in their "Researches on Solar Physics," that if the photosphere of the sun be the plane of condensation of gaseous matter, the plane may be found to be subject to periodical elevations and depressions, and that at the epoch of minimum sun-spot frequency the plane might be uplifted very high in the solar atmosphere, so that there was comparatively little cold absorbing atmosphere above it, and therefore great difficulty in forming a spot.

Changes in the relative thickness of C and F.

This suggestion is one of great value; and, as I pointed out in my previous paper, its accuracy can fortunately now be tested. It may happen, however, that in similar periodical fluctuations the chromosphere may be carried up and down with the photosphere; and I have already evidence that possibly such a state of things may have occurred since 1860, for I do not find the C and F Fraunhofer lines of the same relative thickness as they were in that year.[1] I am waiting to make observations with the large Steinheil spectroscope before I consider this question settled. But the well-known great thickness of the F line in Sirius and other stars will point out the excessive importance of such observations as a method of ascertaining not only the physical constitution, but the actual pressures of the outer limits of stellar atmospheres, and of the same atmosphere at different epochs. And when other spectra have been studied as we have now studied hydrogen, additional means of continuing similar researches will be at our command; indeed a somewhat careful examination of the spectra of the different classes of stars, as defined by Father Secchi, leads me to believe that several broad conclusions are not far to seek; and I hope soon to lay them before the Royal Society.

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[1] I have learnt, after handing this paper in to the Royal Society, that in Ångström's map the C and F lines are nearly of the same breadth : this I had gathered from observations made with my own spectroscope.

First attempts to see the prominences.

For some time past I have been engaged in endeavouring to obtain a sight of the prominences, by using a very rapidly oscillating slit; but although I believe this method will eventually succeed, the spectroscope I employ does not allow me to apply it under sufficiently good conditions, and I am not at present satisfied with the results I have obtained.

Second attempt.

Hearing, however, from Mr. De la Rue, on February 27th, that Mr. Huggins had succeeded in anticipating me by using absorbing media and a wide slit (the description forwarded to me is short and vague), it immediately struck me, as possibly it has struck Mr. Huggins, that the wide slit is quite sufficient without any absorptive media; and during the last few days I have been perfectly enchanted with the sight which my spectroscope has revealed to me. The solar and atmospheric spectra being hidden, and the image of the wide slit alone being visible, the telescope or slit is moved slowly, and the strange shadow-forms flit past. Here one is reminded, by the fleecy, infinitely delicate cloud-films, of an English hedge-row with luxuriant elms: here of a densely interwined tropical forest, the intimately interwoven branches threading in all directions, the prominences generally expanding as they mount upwards, and changing slowly, indeed almost imperceptibly. By this method the smallest details of the prominences and of the chromosphere itself are rendered perfectly visible and easy of observation.[1]

ADDENDUM.—Received March 17, 1869.

Since the foregoing paper was written, I have had, thanks to the somewhat better weather, some favourable opportunities for continuing two of the lines of research more especially alluded to in it; I refer to the method I had adopted for viewing the prominences, and to the injection of sodium, magnesium, &c. into the chromosphere.

With regard to seeing the prominences, I find that, when the sky is free from haze, the views I obtain of them are so perfect that I have not thought it worth while to remount the oscillating slit. I am, however, collecting red and green and violet glass, of the required absorptions, to construct a rapidly revolving wheel, in which the percentages of light of each colour may be regulated. In this way I think it possible that we may in time be able to see the prominences as they really are seen in an eclipse, with the additional advantage that we shall be able to see the sun at the same time, and test the connection or otherwise between the prominences and the surface-phenomena.

Character of the chromosphere.

Although I find it generally best for sketching purposes to have the open slit in a radial direction, I have lately placed it at a tangent to the limb, in order to study the general outline of the chromosphere, which in a previous communication I stated to be pretty uniform, while M. Janssen has characterized it as "*à niveau fort inégal et tourmenté.*" My opinion is now that perhaps the mean of these two descriptions is, as usual, nearer the truth, unless the surface changes its character to a large extent from time to time. I find, too, that in different parts the outline varies: here it is undulating and billowy; there it is ragged to a degree, flames, as it were, darting out of the general surface, and forming a ragged, fleecy, interwoven outline, which in places is nearly even for some distance, and, like the billowy surface, becomes excessively uneven in the neighbourhood of a prominence.[1]

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[1] See Note D.

The prominences.

According to my present limited experience of these exquisitely beautiful solar appendages, it is generally possible to see the whole of their structure; but sometimes they are of such dimensions along the line of sight that they appear to be much denser than usual; and as there is no longer under these circumstances any background to the central portion, only the details of the margins can be observed, in addition to the varying brightnesses.

Moreover it does not at all follow that the largest prominences are those in which the intensest action, or the most rapid change, is going on,—the action as visible to us being generally confined to the regions just in, or above, the chromosphere, the changes arising from violent uprush or rapid dissipation, the uprush and dissipation representing the birth and death of a prominence. As a rule, the attachment to the chromosphere is narrow and is not often single; higher up, the stems, so to speak, intertwine, and the prominence expands and soars upward until it is lost in delicate filaments, which are carried away in floating masses.

Since last October, up to the time of trying the method of using the open slit, I had obtained evidence of considerable changes in the prominences from day to day. With the open slit it is at once evident that changes on the small scale are continually going on; it was only on the 14th inst. that I observed any change at all comparable in magnitude and rapidity to those already observed by M. Janssen.

About 9^h 45^m on that day, with a tangential slit I observed a fine dense prominence near the sun's equator, on the eastern limb. I tried to sketch it with the slit in this direction; but its border was so full of detail, and the atmospheric conditions were so unfavourable, that I gave up the attempt in despair. I turned the instrument

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[1] See Note E.

THIRD PAPER.

round 90° and narrowed the slit, and my attention was at once taken by the F line; a single look at it taught me that an injection into the chromosphere and intense action were taking place. These phenomena I will refer to subsequently.

A prominence observed to change its form.

At $10^h 50^m$, when the action was slackening, I opened the slit; I saw at once that the dense appearance had all disappeared, and cloud-like filaments had taken its place. The first sketch, embracing an irregular prominence with a long, perfectly straight one, which I called A, was finished at $11^h 5^m$, the height of the prominence being $1' 5''$, or about 27,000 miles. I left the Observatory for a few minutes; and on returning, at $11^h 15^m$, I was astonished to find that part of the prominence A had entirely disappeared; not even the slightest track appeared in its place: whether it was entirely dissipated, or whether parts of it had been wafted towards the other part, I do not know, although I think the latter explanation the more probable one, as the other part had increased.

Behaviour of the F *line* (Hβ).

We now come to the other attendant phenomena. First, as to the F line. In my second paper, under the above title, I stated that the F line widens as the sun is approached, and that sometimes the bright line seems to extend on to the sun itself, sometimes on one side of the F line, sometimes on the other.

Dr. Frankland and myself have pointed out, as a result of a long series of experiments, that the widening out is due to pressure, and apparently not to temperature *per se*;[1] the F line near the vacuum-point is thin, and it widens out on both sides (I do not say to the same extent) as the pressure is increased. Now, in the absence of any disturbing cause, it would appear that when the wider line shows itself on the sun on one side of the F line, it should at the same time show itself on the other; this, however, *it does not always do.* I have now additional evidence to adduce on this point, and

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[1] See the Laboratory work further on.

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this time in the prominence line itself, off the sun. In the prominence to which I have referred, the F bright line underwent the most strange contortions, as if there were some disturbing cause which varied the refrangibility of the hydrogen line under certain conditions and pressures.

The D line of hydrogen (?) also once bore a similar appearance.

Secondly, as to the other phenomena which accompanied this strange behaviour of the F line, and were apparently the cause of it.

In the same field of view with F, I recognized the barium line at 1989·5 of Kirchhoff's scale.

Behaviour of the lines of magnesium, barium, nickel, and iron.

Passing on, the magnesium lines and the inclosed nickel-iron line were visible in the chromosphere. The magnesium was projected higher into the chromosphere than the barium, and the nickel or iron was projected higher than the magnesium. I carefully examined whether the other iron-lines were visible in the spectrum of the chromosphere; they were not.

I also searched for the stronger barium lines in the brighter portion of the spectrum; but I did not find them, probably owing to the feeble elevation of the barium-vapour above the general level of the photosphere, which made the observation in this region a very delicate one.

I detected another chromosphere-line very near the iron-line at 1569·5 (on the east side of it).

The sodium lines were also visible.

Unfortunately clouds prevented my continuing these interesting observations: but the action was evidently toning down.

An uprush.'

Here, then, we have an uprush of
Barium,
Magnesium,
? Nickel,
and an unknown substance
from the photosphere into the chromosphere, and with the

uprush a *dense* prominence; accompanying the uprush we have changes of an enormous magnitude in the prominence; and as the uprush ceases the prominence melts away.

As stated in the former part of this paper, the barium and magnesium lines were thinner than the corresponding Fraunhofer lines. In connection with this subject, I beg to be allowed to state that I have commenced a careful comparison of Kirchhoff's map with the recently published one of Ångström. From what I have already seen, I believe other important conclusions, in addition to that before alluded to, may be derived from this comparison; but I hesitate to say more at present, as I have not yet been able to compare Ångström's maps with the sun itself, or to examine the angular diameters of the sun registered at Greenwich during the present century.

Fourth hydrogen line found.

On the 14th inst. I also succeeded in detecting the hydrogen line in the extreme violet in the spectrum of the chromosphere.

FOURTH PAPER.

Spectroscopic Observations of the Sun. No. IV.—By J. NORMAN LOCKYER, F.R.A.S. From the Proceedings of the Royal Society, No. 111, 1869. Received April 14, 1869.[1]

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I BEG to lay before the Royal Society very briefly the results of observations made on the 11th instant in the neighbourhood of a fine spot, situated not very far from the sun's limb.

C *and* F *bright on the sun.*

I. Under certain conditions the C and F lines may be observed *bright on the sun*,[2] and in the spot-spectrum also, as in prominences or in the chromosphere.

II. Under certain conditions, although they are not observed as bright lines, the corresponding Fraunhofer lines are blotted out.

Changes of refrangibility show the effect of motion.

III. The accompanying changes of refrangibility of the lines in question show that the absorbing material moves upwards and downwards as regards the radiating material, and that these motions may be determined with considerable accuracy.[3]

IV. The bright lines observable in the ordinary spectrum are sometimes interrupted by the spot-spectrum, *i.e.* they are only visible in those parts of the solar spectrum near, and away from, spots.

V. The C and F lines vary excessively in thickness over and near a spot, and on the 11th in the deeper portion of the spot they were much thicker than usual.

VI. Stars, in the spectrum of which the absorption-lines of hydrogen are absent, may either have their chromospheric light radiated from beyond the limb just balanced by the light absorbed by the chromosphere on the disc, or they may come under the condition referred to in II., either absolutely or on the average.

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[1] *Proc. R.S.*, vol. xvii. p. 415. [2] See Note F. [3] See Note G.

ADDENDUM.—Received April 29, 1869.

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Since the date on which the foregoing paper was written, I have obtained additional evidence on the points referred to. I beg therefore to be permitted to make the following additions to it.

The possibility of our being able to determine the velocity of movements of uprush and downrush taking place in the chromosphere depends upon the alterations of wave-length observed.

It is clear therefore that a mere uprush or downrush at the sun's limb will not affect the wave-length, but that if we have at the limb cyclones, or backward or forward movements, the wave-length will be altered; so that we may have:—

Upward and downward movements seen near the centre, and backward and forward near the limb of the sun.

I. An alteration of wave-length near the centre of the disc, caused by upward or downward movements.

II. An alteration of wave-length close to the limb, caused by backward or forward movements.

If the hydrogen-lines were invariably observed to broaden out on both sides, the idea of movement would require to be received with great caution; we might be in presence of phenomena due to greater pressure, both when the lines observed are bright or black upon the sun; but when they widen out sometimes on one side, sometimes on the other, and sometimes on both, this explanation appears to be untenable, as Dr. Frankland and myself in our researches at the College of Chemistry have never failed to observe a widening out on both sides the F line when the pressure of the gas has been increased.

On the 21st I was enabled to extend my former observations.

On that day the spot, observations of which form the subject of the paper, was very near the limb; as this was the first opportunity of observing a fine spot under such circumstances I had been able to utilize, I at once commenced work upon it. The spot was so near the limb that

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its spectrum and that of the chromosphere were both visible in the field of view.

The spot-spectrum was very narrow, as the spot itself was so greatly foreshortened; but the spectrum of the chromosphere showed me that the whole adjacent limb was covered with prominences of various heights all blended together.

Further, the prominences seemed fed, so to speak, from, apparently, the preceding edge of the spot; for both C, F, and the line near D, *were magnificently bright on the sun itself*, the latter especially striking me with its thickness and brilliancy.

In the prominences C and F were observed to be strangely gnarled, knotty, and irregular, and I thought at once that some "injection" must be taking place. I was not mistaken. On turning to the magnesium lines I saw them far above the spectrum of the limb and unconnected with it.

A portion of the upper layer of the photosphere had in fact been lifted up beyond the usual limits of the chromosphere, and was there floating cloud-like.

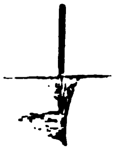
The vapour of sodium was also present in the chromosphere, though not so high as the magnesium, or unconnected with the spectrum of the limb, and, as I expected, with such a tremendous uplifting force, I saw the iron lines (for the first time) in the spectrum of the chromosphere.

My observations commenced at 7.30 A.M.; by 8.30 there was comparative quiet.

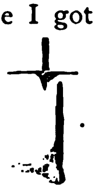
At 9.30 the action had commenced afresh; there was now a single prominence.

Behaviour of F in a prominence.

At the base of the prominence I got this appearance:



. Higher up this:



. Here I may be

FIG. 144.

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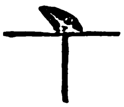
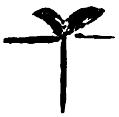
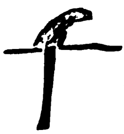
permitted to recall the observation made on March 14th, in which a slight movement of the slit gave me first , then , and finally ;—all these appearances being due to cyclonic action.

FIG. 145.

On the following side of the spot, at about 10 A.M., I observed that the F line had disappeared; at the point of disappearance there appeared to be an elongated, brilliantly illuminated lozenge lying across it at right angles, as if the spectroscope were analysing the light proceeding from a cyclone of hydrogen on the sun itself, but so near the limb that the rotatory motion could be detected.

The next observations I have to lay before the Royal Society were made on the 27th inst. Careful observations on the 25th and 26th revealed nothing remarkable except that the chromosphere was unusually uniform.

On the 27th a fine spot with a long train of smaller ones and faculæ was well on the disc. The photosphere in advance of the spot, and the large spot itself, showed no alteration from the usual appearance of the hydrogen-lines; but in the tails of the spot the case was widely different.

The F line, at which I worked generally, as the changes of wave-length are better seen, was as irregular as on the former occasions.

I. It often stopped short of one of the small spots, swelling out prior to disappearance.

II. It was invisible in a facula between two small spots.

F *widened and changed into a bright line on a spot.*

III. *It was changed into a bright line, and widened out on both sides two or three times* IN THE VERY SMALL SPOTS.

IV. Once I observed it to become bright *near* a spot, and to expand over it on both sides.

V. Very many times near a spot it widened out, sometimes considerably, on the less refrangible side.

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VI. Once it extended as a bright line without any thickening over a small spot.

F seen half bright and half dark.

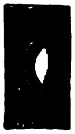
VII. Once it put on this appearance:  bright.

FIG. 146.

VIII. I observed in it all gradations of darkness.

IX. When the bright and dark lines were alongside, the latter was always the less refrangible.

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Spectroscopic Observations of the Sun. No. V.—By J. NORMAN LOCKYER, F.R.S. From the Proceedings of the Royal Society, No. 115, 1869. Received July 8, 1869.[1]

FIFTH PAPER.

SINCE the date of my last communication under the above title, the weather has, if possible, been worse for telescopic work than during the winter and spring; my opportunities of observation, therefore, have been very limited; still the sun has occasionally been in such a disturbed state, and our atmosphere has at times been so pure, that several new facts of importance have come out.

I will state them here as briefly as possible, reserving a discussion of them and my detailed observations for a future occasion.

Rates of movement in cyclones.

I. The extreme rates of movement in the chromosphere observed up to the present time are:—

Vertical movement . . . . . . 40 miles a second.
Horizontal or cyclonic movement. 120 „

II. I have carefully observed the chromosphere when spots have been near the limb. The spots have sometimes been accompanied by prominences, at other times they have not been so accompanied. Such observations show that we may have spots visible without prominences in the same region, and prominences without spots; but I do not say that a spot is not accompanied by a prominence *at some stage of its life*, or that it does not result from some action which, in the majority of cases, is accompanied by a prominence.

III. At times, when a prominence is seen bright on the sun itself, the bright F line varies considerably, both in thickness and brilliancy, within the thickness of the dark line. The appearances presented are exactly as if we were looking at the prominences through a grating.

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[1] *Proc. R.S.*, vol. xviii. pp. 74 and 118.

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IV. Bright prominences, when seen above spots on the disc, if built up of other substances besides hydrogen, are indicated by the bright lines of those substances in addition to the lines of hydrogen. The bright lines are then seen very thin, situated centrally (or nearly so) on the broad absorption-bands caused by the underlying less-luminous vapours of the same substances.

An absorption line corresponding to D_3 *detected.*

V. I have at last detected an absorption-line corresponding to the orange line in the chromosphere. Father Secchi states[1] that there is a line corresponding to it much brighter than the rest of the spectrum. My observation would seem to indicate that he has observed a bright line less refrangible than the one in question, which bright line is at times excessively brilliant. It requires absolutely perfect atmospheric conditions to see it in the ordinary solar spectrum. It is best seen in a spot-spectrum when the spot is partially covered by a bright prominence.

VI. In the neighbourhood of spots the F bright line is sometimes observed considerably widened out in several places, as if the spectroscope were analysing injections of hydrogen at great pressure in very limited regions into the chromosphere.

VII. The brilliancy of the bright lines visible in the ordinary solar spectrum is extremely variable. One of them, at 1871·5, and another, at 1529·5 of Kirchhoff's scale, I have detected in the chromosphere at the same time that they were brilliant in the ordinary solar spectrum.

VIII. Alterations of wave-length have been detected in the sodium, magnesium, and iron lines in a spot-spectrum. In the case of the last substance, the lines in which the alteration was detected were *not* those observed when iron (if we accept them to be due to iron alone) is injected into the chromosphere.

IX. When the chromosphere is observed with a tangential slit, the F bright line close to the sun's limb

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[1] *Comptes Rendus*, 1869, 1^{r}. sem., p. 358.

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shows traces of absorption, which gradually diminish as the higher strata of the chromosphere are brought on to the slit, until the absorption-line finally thins out and entirely disappears. The lines of other substances thus observed do not show this absorption.

X. During the most recent observations I have been able to detect traces of magnesium and iron in nearly all solar latitudes in the chromosphere. If this be not merely the result of the good definition lately, it would indicate an increased general photospheric disturbance as the maximum sun-spot period is approached. Moreover I suspect that the chromosphere has lost somewhat of its height.

I append a list of the bright lines,[1] the positions of which in the chromosphere I have determined absolutely, with the dates of discovery, remarking that in the case of C and F my observations were anticipated by M. Janssen :—

List of chromospheric bright lines.

Hydrogen.

C.	October 20, 1868.
F.	October 20, 1868
near D.	October 20, 1868.[2]
near G.	December 22, 1868.
h.	March 14, 1869.

Sodium.

D.	February 28, 1869.

Barium.

1989·5.[3]	March 14, 1869.
2031·2.	July 5, 1869.

Magnesium and included line.

$$\left.\begin{array}{l} b^1 \\ b^2 \\ b^3 \\ b^4 \end{array}\right\} \text{February 21, 1869.}$$

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[1] See Note H.

[2] *Hydrogen?*

[3] This reference is to Kirchhoff's scale.

List of chromospheric bright lines.

Other Lines.

Iron.	1474.[1]	June 6, 1869.
?	1515·5.	June 6, 1869.
Bright line . .	1529·5.	July 5, 1869.
?	1567·5.	March 6, 1869.
?	1613·8.	June 6.
Iron.	1867·0.	June 26.
Bright line . .	1871·5.	"
Iron.	2001·5.	"
?	2003·4.	"
? band or line near black line, very delicate . . .	2054·0.	July 5.

I have seen other lines besides these at different times, but I do not include them, as their positions have not been determined absolutely.

I refrain from dwelling on this list at present, except to point out that, taking iron as an instance, and assuming that the iron lines mapped by Ångström and Kirchhoff are due to iron only, I have only been able, up to the present time, to detect three lines out of the total number (460) in the spectrum of the lower regions of the chromosphere, —a fact full of promise as regards the possible results of future laboratory work. The same remark applies to magnesium and barium.

Widening of sodium lines.

Dr. Frankland and myself have determined that the widening out of the sodium line in the spectrum of a spot which I pointed out in 1866, and then stated to be possibly an evidence of greater absorption, indicates a greater absorption due to greater pressure.

The continuous widening out of the sodium-line in a spot must therefore be regarded as furnishing an additional argument (if one were now needed) in favour of the theory of the physical constitution of the sun first put forward by Dr. Frankland and myself—namely, that the chromosphere

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[1] See Note I.

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and the photosphere form the true atmosphere of the sun, and that under ordinary circumstances the absorption is continuous from the top of the chromosphere to the bottom of the photosphere, at whatever depth from the bottom of the spot that bottom may be assumed to be.

This theory was based upon all our observations made from 1866 up to the time at which it was communicated to the Royal Society and the Paris Academy of Sciences, and has been strengthened by all our subsequent work; but several announcements made by Father Secchi to the Paris Academy of Sciences and other learned bodies are so opposed to it, and differ so much from my own observations, that it is necessary that I should refer to them, and give my reasons for still thinking that the theory above referred to is not in disaccord with facts. At the same time I must state that Father Secchi does not combat this theory; indeed it is not to be gathered from any of his communications that he has seen any of the papers communicated by myself to the Royal Society.

Secchi's statements.

Father Secchi states that the chromosphere is often separated from the photosphere, and that between the chromosphere and the photosphere there exists a stratum giving a continuous spectrum, which he considers to be the base of the solar atmosphere, and in which he thinks that the inversion of the spectrum takes place.[1]

With regard to the first assertion, I may first state that all the observations I have made have led me to a contrary conclusion. Secondly, in an instrument of comparatively small dispersive power, such as that employed by Father Secchi, in which the widening out of the F line at the base of the chromosphere is not clearly indicated, it is almost impossible to determine, by means of the spectroscope, whether the chromosphere rests on the sun or not, as the chromosphere is an envelope and we are not dealing merely with a section. But an instrument of great dispersive power can at once settle the question; for since the F line widens out with pressure, and as the pressure increases as the sun is approached, the continuous curvature of the F line must indicate really the spectrum of a section; and if the chromosphere were suspended merely at a certain height above the photosphere, we should not get a widening due to pressure: but we always do get such a widening.

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[1] See Note K.

With regard to the second assertion, I would remark that if such a continuous-spectrum-giving envelope existed, I entirely fail to see how it could be regarded as a region of selective absorption. Secondly, my observations have indicated no such stratum, although injections of sodium, magnesium, &c. into the chromosphere not exceeding the limit of the sun's limb by 2″ have been regularly observed for several months past. To-day I have even detected a low level of barium in the chromosphere not 1″ high. This indicates, I think, that my instrument is not lacking in delicacy; and as I have never seen anything approaching to a continuous spectrum when my instrument has been in perfect adjustment, I am inclined to attribute the observation to some instrumental error. Such a phenomenon might arise from a local injection of solid or liquid particles into the chromosphere, if such injection were possible. But I have never seen such an injection. If such an occurrence could be observed, it would at once settle that part of Dr. Frankland's and my own theory, which regards the chromosphere as the last layer of the solar atmosphere; and if it were possible to accept Father Secchi's observation, the point would be settled in our favour.

The base of the atmosphere below the spot.

The sodium experiments to which I have referred, however, and the widening out of the lines in the spot-spectra, clearly indicate, I think, that the base of the atmosphere is below the spot, and not above it. I therefore cannot accept Father Secchi's statement as being final against another part of the theory to which I have referred—a conclusion which Father Secchi himself seems to accept in other communications.

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Father Secchi remarks also that the F line is produced by the absorption of other bodies besides hydrogen, because it never disappears. This conclusion is also negatived by my observations; for it has very often been observed to disappear altogether and to be replaced by a bright line. At times, as I pointed out to the Royal Society some months ago, when a violent storm is going on accompanied by rapid elevations and depressions of the prominences, there is a black line on the less refrangible side of the bright one; but this is a phenomenon due to a change of wave-length caused by the rapid motion of the hydrogen.

***A black line on less refrangible side of* F.**

With regard to the observation of spot-spectra, I find that every increase of dispersive power renders the phenomenon much more clear, and at the same time more simple. The selective absorption I discovered in 1866 comes out in its most intense form, but without any of the more complicated accompaniments described by Father Secchi. I find, however, that by using three prisms this simplicity vanishes to a great extent. We get portions of the spectrum here and there abnormally bright, which have given rise doubtless to some of the statements of the distinguished Roman observer; but the bright lines, properly so-called, are as variable as they are in any other part of the disc, but not much more so. I quite agree that the "interpretation" of sun-spot phenomena to which Father Secchi has referred,[1] which ascribes the appearances to anything but selective *plus* general absorption, is erroneous. But as I was not aware that it had ever been propounded, I can only refer to my own prior papers in support of my assertion, and to Mr. Huggins' endorsement of my observations, which were communicated to the Royal Society some three years ago.

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[1] *Comptes Rendus*, 1869, 1r. sem. p. 764. I have since found that Father Secchi was combating some statements made by Professor Respighi, whose paper I had not then seen.

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This paper, which appeared in the *Comptes Rendus* of the French Academy, as well as in the *Proceedings of the Royal Society*, was followed by a discussion between Father Secchi and myself, in the *Comptes Rendus* of the Paris Academy, which, in justice to that observer, must be given here. Father Secchi's reply[1] runs as follows:—

"ROME, *ce* 30 *juillet*, 1869.

"Absent de l'Observatoire pendant quelques jours, c'est seulement hier que j'ai eu connaissance de la Note de M. Lockyer, insérée dans les *Comptes Rendus* du 12 juillet. Dans cette Note, M. Lockyer représente mes résultats sur la constitution du Soleil comme *très-opposés aux siens*, mais il avoue en même temps que *je ne combats pas sa théorie;* il en conclut que *je n'ai pas connaissance des Mémoires présentés par lui* à la Société Royale. Pour bien juger de cette divergence, il faut séparer deux choses, les faits les hypothèses. 1° M. Lockyer conteste des faits observés par moi : je ferai donc voir quelle est la cause pour laquelle il n'a pas pu les vérifier ; 2° il expose une théorie sur laquelle nous sommes partiellement d'accord, et que je rejette en partie ; mais ce point est tout à fait secondaire pour moi. Pour ce qui regarde ses travaux, j'avoue que je n'ai connaissance que de ceux qui ont été publiés dans les *Comptes Rendus* ou dans *les Mondes*. Ces derniers sont sans doute des extraits bien imparfaits, et les énonciations sont si vagues et si générales qu'elles se prêtent à toute interprétation, et établiraient la priorité de toute découverte possible. Que conclure, par exemple, de ces expressions que, sous certaines conditions, on voit telle ou telle chose (p. 122, premier et deuxième alinéa) ? Il faut établir nettement ces conditions, car tout reste possible. Ainsi, lorsque M. Lockyer affirme m'avoir précédé dans les observations des injections de magnésium ; on ne peut pas dire *jusqu'à quel point* cela est exact, car je ne trouve pas encore publiés régulièrement les détails de son observation, et, du reste, *j'ai clairement vu et démontré que seulement une*

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[1] *Comptes Rendus*, t. lxix., p. 31, *et seq.*, August 2, 1869.

ligne du magnésium est renversée, et que l'autre ligne brillante occupe l'espace intermédiaire des deux plus voisines. Ce sont des faits capitaux dans notre sujet, qu'on ne peut pas énoncer d'une manière aussi vague que le fait M. Lockyer. Il est donc grandement à désirer que M. Lockyer communique ses résultats avec plus de détails, pour établir ses droits sur ce point et sur les autres.

Only one line of magnesium reversed.

" Venons maintenant aux faits qu'il met en doute. Il conteste d'abord ce que j'ai dit sur la couche à spectre continu que j'ai vue dans le Soleil, entre le bord et la chromosphère ; et, dans sa critique, il mêle des idées théoriques que je n'accepte pas. Quant au fait, je l'ai assez souvent vu et revu pour n'en pouvoir pas douter ; j'ai même détaillé toutes les circonstances dans lesquelles il se manifeste et *les précautions* à employer pour le voir, de sorte que je n'ai aucun doute, et je n'ai pas à revenir sur ce point. Dans cette matière, une observation négative ne fait pas autorité, et ce sont ces résultats négatifs qui ont tant retardé la découverte de ces phénomènes si faciles à voir.

The "couche à spectre continu."

" M. Lockyer rejette son insuccès sur la petitesse de mon instrument : dans une autre question parallèle à celle-ci, on a aussi commencé par cette objection, mais on a fini par admettre le résultat : il en sera ainsi de la présente ; car un spectroscope qui fait voir *toutes* les raies de Kirchhoff, et qui possède une telle dispersion et une telle force réfractive, que le rayon sorti des trois prismes est parallèle au rayon entrant, un instrument auquel on peut appliquer de plus un prisme à vision directe de la force de deux autres prismes, de manière à en faire en réalité un instrument à cinq prismes, ne me paraît pas un instrument faible pour cette espèce de recherches.

The power of Secchi's instrument.

" Je crois, au contraire, que le défaut est du côté de M. Lockyer. Je ne connais pas en détail sa manière d'observer, ni son instrument, mais il me semble, par la description de ses résultats, qu'il ne grossit pas, ou qu'il grossit peu l'image du Soleil. Dans ce cas, il est évident qu'il ne pourra pas séparer la lumière de la couche en question de

Necessity of enlarging the image of the Sun.

la lumière de couches qui l'environnent, car cette couche aura à peine l'épaisseur de la largeur de la fente dans l'image directe d'une lunette de neuf pouces, comme la mienne. Au contraire, avec mon système d'observation, en grossissant convenablement l'image directe, on donne plus de largeur à la couche, et il devient possible de la séparer des autres.

With a large image F is not widened.

"Je crois encore que M. Lockyer se méprend, lorsqu'il dit que mon instrument est incapable d'élargir la raie F à la base, et que cette méprise tient à la même cause, la petitesse de son image directe. En effet, en employant l'image directe, j'ai vu la raie F très-brillante et en forme de fer de lance, et en ouvrant un peu la fente on voyait toute la protubérance, et on relevait son contour ordinairement conique. Mais ces apparences s'évanouissent en grossissant l'image solaire, car la protubérance acquiert alors une hauteur linéaire plus grande, et une largeur qui surpasse plusieurs fois la largeur de la fente.

Doubtful importance of these enlargements.

"M. Lockyer insiste beaucoup sur l'élargissement de cette raie F à la base, mais j'ai des doutes sur l'importance qu'il lui attribue. En effet, les lignes lumineuses F, C, ou autres que nous voyons, ne sont pas des objets réels: elles sont seulement l'image de la fente éclairée par la lumière d'une certaine réfrangibilité. Cette image doit, pour une lumière homogène, être terminée par deux lignes parallèles, lorsque l'image de la protubérance a une largeur supérieure à la largeur de la fente. C'est réellement ce que je vois habituellement, car la raie F se présente avec une longueur apparente de 1 à 2 centimètres:[1] elle traverse parfois toute la largeur du spectre. Cette ligne est toujours terminée par une pointe effilée, comme les autres raies C et D_3. S'il y a donc une dilatation à sa base, elle peut provenir de deux causes: 1° d'une irradiation, due à la vivacité de la lumière, qui y produit une expansion apparente; 2°

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[1] Cette manière d'énoncer la grandeur apparente est sans doute vulgaire, mais elle est assez commode pour donner une idée de la grandeur du phénomène observé.

d'une différence de réfrangibilité, qui, ajoutant de nouveaux rayons à droite et à gauche de la raie, l'élargit aussi. Ces deux causes sont également possibles et probables, et il reste à trouver la véritable.

Appearance of C.

"C'est d'abord un fait bien constaté que l'intensité de la lumière des protubérances n'est pas toujours la même et que l'irradiation est parfois assez forte pour donner à la raie C la forme de coin paraissant plus dilatée là où elle est plus vive : mais, en mettant soigneusement au point focal de vision la fente, et surtout en amplifiant l'image de la protubérance, comme je le fais habituellement, on trouve toujours une portion rectiligne terminée par une pointe. Et s'il y a un nuage suspendu, on voit nettement le milieu du nuage rectiligne et les extrémités en pointe effilée. Cette pointe, du reste, peut bien s'expliquer, soit par la densité du nuage soit par l'intensité de la lumière.

Fig.147

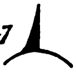
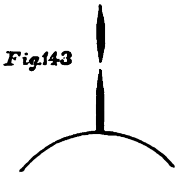


Fig.143



"Je crois donc que, si M. Lockyer grossit convenablement ses images, il verra disparaître ces courbures desquelles il tire beaucoup de conclusions, qui sont pour cette raison au moins douteuses, et qui pourraient bien être, ou un effet de la forme même de la protubérance qui serait plus étroite que la fente elle-même, ou un effet d'irradiation.

The objections to the "couche à spectre continu" invalid.

"M. Lockyer continue ses objections par des considérations théoriques, et dit que, *si la chromosphere était suspendue à une certaine distance de la photosphère, nous ne pourrions trouver un élargissement dû à la pression.* J'avoue que je ne vois pas la légitimité de cette conclusion ; car, même en admettant la chromosphère suspendue, elle devrait toujours suivre la loi du décroissement de densité que subit l'atmosphère solaire dans laquelle elle nage. Il faut bien remarquer que cette structure des masses suspendues dans une atmosphère ne résulte pas des observations spectroscopiques, mais bien des observations des éclipses ; et il est impossible

The chromosphere not the last layer of the solar atmosphere.

d'admettre que ces nuages ou ces colonnes inclinées puissent rester suspendues, sans un milieu qui les supporte et qui soit different d'elles-mêmes. M. Lockyer, qui n'admet pas ce milieu, où nagent les protubérances, trouve sans doute inadmissibles bien des choses, mais à notre tour nous n'admettons pas son hypothèse, que la *chromosphère soit la dernière couche de l'atmosphère solaire.*

" Mais laissant de côté la théorie et revenant aux faits, il me semble que, pendant que M. Lockyer rejette mes résultats, il vient réellement les appuyer par ses observations. En effet, il dit avoir vu partout de nombreuses émanations de sodium de 1 à 2 secondes, et d'autres métaux, etc. Or, je demande comment il a vu ces émanations? Sans doute par le renversement des raies ou par l'absence des raies noires: or c'est là précisément le fait contesté; c'est-à-dire qu'il y a au bord du Soleil un filet très-mince, où un grand nombre de ces raies et parfois toutes les plus faibles disparaissent. M. Lockyer appuie donc mon observation en la combattant, et la seule différence entre nous serait qu'il a eu occasion de voir des phénomènes plus saillants, pendant que j'ai vu des phénomènes plus compliqués. M. Lockyer n'admet pas mes résultats, parce qu'il les croit incompatibles avec la théorie: il ajoute que cela *serait seulement possible si des particules solides et liquides pouvaient être injectées dans la chromosphère.* Nous ne voyons pas cette impossibilité, et d'ailleurs il y a peut-être bien des manières d'interpréter un tel phénomène; du reste, la formation d'une théorie définitive quelconque me paraît prématurée, parce que nous n'avons pas assez de faits encore bien constatés.

The injection of solid or liquid particles not impossible.

" M. Lockyer n'admet pas même ce que j'ai trouvé dans le spectre des taches, et cherche à mettre mes résultats en opposition avec les siens; mais ici nous sommes, en partie au moins, parfaitement d'accord. M. Lockyer dit: *L'élargissement des raies dans le spectre indique clairement, je pense, que la base de l'atmosphère est au-dessous des taches et non au-dessus; je ne peux donc admettre que les affirmations du*

P. Secchi soient des arguments définitifs contre une partie de la théorie que je viens de rappeler, etc. Autant qu'il m'est possible de comprendre cette phrase, nous ne sommes pas en désaccord ici. En effet, j'ai admis moi-même que, dans les taches, il se produit une absorption plus forte par la raison qu' *étant des cavites remplies de la matière de l'atmosphère transparente du Soleil, elles constituent une couche plus profonde*, ce qui implique bien que la base de l'atmosphère soit au-dessous des taches, comme le veut M. Lockyer. Cela est encore plus clair dans l'hypothèse de Wilson et Herschel, d'après laquelle la photosphère ne serait qu'un brouillard lumineux suspendu dans l'atmosphère transparente, hypothèse que jusqu'ici je juge la plus probable. Du reste, je ne sais pas quelles sont *les affirmations* auxquelles il fait ici allusion, surtout après avoir déclaré que je ne combats pas sa théorie.

The spots are cavities of greater absorption.

"M. Lockyer conteste aussi ma conclusion, que la raie F puisse être composée, car il dit qu'il n'a pas observé les faits que j'ai indiqués. Cela me paraît inconcevable, car lui-même, à la page 122, n° IX, il dit expressément: *lorsque la ligne brillante* (F) *et la ligne noire se trouvaient côté à côté, la dernière était toujours la moins réfrangible.* Or c'est là justement ce que j'ai observé et énoncé de cette manière, que près du bord la ligne brillante ne remplit pas toute la largeur de la ligne noire, mais en laisse une partie noire du côté du rouge. Le fait est au fond le même, et si j'ai pu le constater, malgré sa délicatesse, cela prouve bien que mon instrument n'est pas insuffisant. Si M. Lockyer a vu la raïe F renversée, cela n'est pas surprenant, car elle doit se renverser comme la raie C, mais il sera bien difficile de constater le renversement total, car la lumière de la raie F est plus faible que celle de la raie C.

Composite nature of F.

"Du reste, il est difficile de juger des détails de ces observations et de trouver la source des discordances entre les deux observateurs, sans connaître à fond le système d'observation employé par M. Lockyer. S'il ne grossit pas convenablement son image, il pourrait bien se faire que tous ces mouvements et ces changements de réfrangibilité des raies, qu'il dit avoir observés, fussent des illusions. J'ai vu fréquemment des mouvements semblables se traduisant quelquefois par une duplication de la raie, mais je les ai attribués à l'agitation de notre atmosphère, et à la chaleur solaire agissant sur la fente, qui peut bien produire des déviations accidentelles des rayons. Ordinairement, ces phénomènes disparaissent en mettant bien au foyer l'appareil. Cela soit dit cependant sans révoquer en doute les assertions de cet observateur éminent, mais seulement pour le mettre en garde contre une cause d'illusion qui a d'autant plus d'influence que les images sont plus petites.

Spot spectra.

"Pour n'être pas trop long, j'omettrai d'autres détails secondaires, et je terminerai par ce qu'il dit sur le spectre observé par moi dans l'intérieur des taches. Ici il ne m'a pas été aussi facile de saisir sa critique, car il dit que '*j'ai décrit des phénomènes très-compliqués, accompagnant ce spectre des taches,*' ce qui voudrait dire qu'en réalité les phénomènes sont plus simples : cependant il ajoute '*qu'avec trois prismes, la simplicité s'évanouit en grande partie :*' cela me paraît une contradiction ; et, du reste, si avec trois prismes j'ai vu des phénomènes compliqués, cela prouve que l'instrument n'est pas mauvais. M. Lockyer paraît contester le fait affirmé par moi, que dans les taches l'intensité relative des différentes parties est notablement altérée car il dit que, dans les taches, les lignes brillantes sont *aussi variables que dans les autres parties du disque.* Si c'était là le sens de la critique, je devrais vraiment regretter qu'il fût arrivé à ce résultat complétement erroné, et je n'aurais pas une haute opinion de son instrument et de sa méthode d'observation, car cette particularité, découverte par moi, est si saillante, qu'elle a pu être vue par un grand nombre d'observateurs, entre lesquels des astronomes assez distingués. Mais peut-être n'ai-je pas bien saisi le sens de l'objection.

"Du reste, pour le moment, ces recherches sont impraticables dans notre climat, et je me réserve de revenir sur ce sujet, dès que la saison moins brûlante le permettra."

FIFTH PAPER.

To these remarks of Father Secchi I sent the following reply[1]:—

"La partie de ma Communication qui a été imprimée dans les *Comptes Rendus* du 12 juillet, et qui a rapport aux observations du P. Secchi, avait pour objet, comme je l'ai clairement indiqué, l'établissement de certains points sur lesquels nos observations n'étaient pas d'accord, afin que d'autres personnes puissent employer la nouvelle méthode d'observation dans les meilleures conditions.

"J'ai donc été un peu surpris du ton de la réponse du P. Secchi, ton que je n'ai pas la moindre intention d'imiter dans le présente Communication.

Secchi's views.

"Je trouve, dans la lettre du P. Secchi:

"1° Qu'il tient encore à 'une couche donnant *un spectre continu*, couche qu'il considère comme *la base de l'atmosphère solaire* et dans laquelle il pense que *s'effectue le renversement* selon la théorie de Kirchhoff;'

"2° Qu'il a des doutes sur l'importance que j'attribue à l'élargissement de la raie F à sa base, sur lequel le Dr. Frankland et moi avons fondé notre estime de la pression de la chromosphère;

"3° Qu'il a aussi des doutes sur les changements de longueur d'onde dans les raies de l'hydrogène que j'ai affirmé être continuellement visibles, sur le Soleil et en dehors de cet astre;

"4° Qu'il soutient encore que la raie F est due à l'absorption de quelqu'autre substance, en outre de l'hydrogène.

"Sur tous ces points, je m'en remets volontiers au jugement de l'avenir. Je puis cependant faire remarquer, quant à ce qui touche le premier de ces points, que quoique je ne voie rien qui ressemble à un spectre continu, je vois positivement des traces d'absorption réduite dans la couche extérieure de la photosphère, et le Dr. Frankland et moi avons montré que l'absorption augmente lorsque les couches inférieures sont mises en action comme dans une tache. Le P. Secchi a écrit récemment (*Comptes Rendus*, 1869, 2°

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[1] *Comptes Rendus*, vol. cit., p. 452, *et seq.*, August 16th, 1869.

FIFTH PAPER.

Contradictory observations.

semestre, p. 41): 'Ayant examiné comparativement le spectre du noyau des taches et celui du bord du disque du côté intérieur, je suis arrivé à la conclusion que ces deux spectres se ressemblent considérablement. *L'élargissement des raies constaté dans le noyau se reproduit près* du bord.' Je ne trouve pas cela, mais il me semble que c'est une contradiction pure et simple avec son assertion relative au spectre continu dans ces régions.

"Quant au second point, dans lequel l'influence de la pression est mise en question, je cite encore (*Comptes Rendus*, 1869, 2_e semestre, p. 41): 'J'ai encore porté mon attention sur la largeur des raies brillantes de la chromosphère et j'ai constaté que, en général, les raies principales sont toutes trois plus larges à la base qu'au sommet, ce qui PROUVE L'INFLUENCE DE LA PRESSION · *exercée par les couches supérieures*.' Ces expressions me frappent, comme étant une nouvelle contradiction, et le P. Secchi devra choisir entre ses assertions opposées. Mais je dois lui rendre la justice d'admettre qu'il avait déjà imaginé qu'on pourrait arriver à quelque résultat au moyen d'expériences de pression; car il avait recommandé d'observer la spectre de l'hydrogène *sous une tres-forte pression*, tandis que le Dr. Frankland et moi avons réussi en opérant *près du point du vide*.

Influence of pressure.

"Laissant donc de côté des points importants sur lesquels, comme je l'ai déjà dit, je m'en remets à la décision de l'avenir, il y en a plusieurs d'une importance moindre, sur lesquels je prie l'Académie de me permettre de dire quelques mots.

Magnesium injections.

"Le P. Secchi dit: 'Lorsque M. Lockyer affirme m'avoir précédé dans les observations des injections de magnésium, on ne peut pas dire jusqu'à quel point cela est exact.' En ce qui touche à la question de priorité, je ne regarderais pas cette remarque comme méritant une réponse; mais comme le P. Secchi paraît jeter un doute sur ma véracité, je le prierai de lire un Mémoire communiqué à la Société Royale le 4 mars 1869, dans lequel il est établi que les premières observations ont été faites les 21 et 28 février.

The question of the single line.

"Quant à son assertion qu'il énonce ainsi : 'J'ai clairement vu et démontré que seulement une ligne du magnésium est renversée et que l'autre ligne brillante occupe l'espace intermédiaire des deux plus voisines,' je me considère comme pleinement justifié en niant l'exactitude de l'observation, et je laisse cette question, comme les autres, à la décision de l'avenir ; je ne m'aventurerais pas jusque-là si une longue série d'expériences, faites au Collége Royale de Chimie, n'avait pas été complétement d'accord avec mes observations télescopiques qui ont déjà été décrites dans les *Comptes Rendus.*

Value of a good theory.

"Le P. Secchi me fait le reproche de mêler la théorie avec les observations. J'avoue franchement qu'il en est ainsi. Je confesse qu'une remarque faite, il y a déjà quelque temps, par M. Faye est toujours présente à mon esprit lorsque je me livre à des observations. Voici cette remarque : 'Une bonne théorie est aussi nécessaire qu'un bon télescope.' Sans une hypothèse qui dirigeait mon travail, j'aurais bien certainement beaucoup moins interrogé le Soleil que je ne l'ai fait, et ce serait une naïveté de dire que, dans une recherche comme celle que nous poursuivons maintenant, il ne convient pas d'observer aveuglément ou au hasard. Par exemple, j'ai commencé par m'appuyer sur la théorie, généralement admise alors, que 'l'absorption avait lieu en dehors de la photosphère,' ce qui est évidemment l'idée actuelle du P. Secchi, comme on le voit par l'extrait d'une de ses dernières Communications que j'ai déjà cité. Mais, en mettant à l'épreuve cette théorie de toutes les manières, j'ai reconnu qu'elle était insoutenable, et je crois pouvoir dire que, si le P. Secchi avait fait comme moi, il y aurait eu moins de contradictions dans ce qu'il a cherché à établir, et aussi qu'il aurait trouvé que cette théorie n'est pas soutenable. Mais j'avoue que la remarque qu'il est trop tôt pour établir une théorie, venant du P. Secchi, m'a un peu étonné, car je trouve un grand nombre de tentatives de ce genre dans ses propres Mémoires sur ce sujet.

Chromosphere not suspended.

"Dans ma première Note, j'établissais que, 'si la chromosphère était suspendue à une certaine distance de la photosphère, nous ne pourrions trouver un élargissement dû à la pression.' Sur ce point, P. Secchi fait remarquer qu'il ne voit pas la justesse de ma conclusion. Cela peut dépendre de ce qu'il n'admet pas que la raie F s'élargit par la pression. Mais ma remarque signifie simplement que, si la chromosphère était mince au lieu d'être épaisse, c'est-à-dire si elle ne s'étendait pas jusqu'à la photosphère, il y aurait moins de place pour que la pression devienne évidente. Il ajoute alors : 'Cette structure des masses suspendues dans une atmosphère . . . . . résulte . . . . des observations des éclipses.' Je demanderai alors où de telles observations de la chromosphère (car les proéminences sont hors de la question) ont été consignées, et comment ces observations, si elles existent, pourraient nous servir dans une telle question.

"Je lis ensuite : 'Il est impossible d'admettre que ces images ou ces colonnes inclinées puissent rester suspendues sans un milieu qui les supporte et qui soit différent d'elles-mêmes.' A cela je répondrai que, si le P. Secchi ou tout autre peut *prouver* l'existence de ce milieu, je l'admettrai bien volontiers ; mais j'aurai toujours présent à l'esprit :

Results of Frankland and Lockyer's experiments.

"1° Que, quoique le Dr. Frankland et moi ayons démontré que la température est assez élevée dans la region des proéminences pour rendre d'autres substances incandescentes s'il y en avait là, le spectroscope ne nous fournit aucun indice de leur existence ;

"2° Que la ténuité des proéminences est excessive ;

"3° Que les proéminences ne sont pas stables, mais sont rapidement entraînées au delà du niveau supérieur de la chromosphère et disparaissent aussi rapidement ;

"4° Qu'il n'y a pas d'hydrogène plus froid au-dessus de la chromosphère, car nous n'aurions alors que la raie F toute seule ;

"5° Que l'hydrogène est le plus léger de tous les gaz.

"Le P. Secchi regarde mes observations d'injections de sodium, de magnésium, etc., *dans la chromosphère*, comme une preuve de l'existence de la couche ou *stratum* à spectre continu qu'il place au-dessous de la chromosphère. Je lui demanderai comment cela est possible. Mes observations, en outre, démontrent, je pense, que les vapeurs de sodium, de magnésium, etc., sont immédiatement placées au-dessous de la chromosphère, et alors comment peuvent-elles donner au-dessus un spectre continu, si elles n'en donnent pas au-dessous ?

Influence of optical power.

"Dans ma dernière Communication j'ai dit qu'en employant trois prismes, les phénomènes des taches pouvaient être aussi compliqués que le P. Secchi les a décrits, mais qu'avec les puissants pouvoirs dispersifs que j'emploie, cette complication disparaît en très-grande partie. A cette occasion le P. Secchi dit que 'je cherche à mettre mes résultats en opposition avec les siens.' Je ferai remarquer que mon but était justement le contraire, et je cite encore un des derniers Mémoires du P. Secchi (*Comptes Rendus*, 1869, 2e. semestre, p. 166), d'une date plus récente que celui dans lequel il établit que le spectre d'une tache est semblable à celui du limbe : 'Il n'y a pas production des raies fondamentales nouvelles, mais seulement un renforcement considérable des raies solaires connues comme déjà existantes.' Sur ce point, je laisserai encore le P. Secchi se mettre d'accord avec lui-même. Quant à ce qui touche spécialement aux raies brillantes, visibles parfois dans les taches, que le P. Secchi regarde comme dues à la radiation du noyau gazeux intérieur du Soleil (*Comptes Rendus*, 1869, 1er. trimestre, p. 165), je puis dire seulement que je n'ai pas vu dans les taches de lignes brillantes qui ne fussent visibles en même temps dans le spectre solaire ordinaire. Il est vrai que, dans les taches, elles sont beaucoup mieux vues.

"En terminant, je dois dire que la méthode d'observation que j'emploie, et dont je regrette de voir que le P. Secchi fait si peu de cas, consiste a observer l'image actuelle du Soleil fournie par un réfracteur de 6·25 pouces d'ouverture et de 100 pouces de longueur focale, ou bien une image agrandie jusqu'à 6 pouces en diamètre, suivant l'état de l'air. Je dirai, en outre, que mon spectroscope est muni d'une série de sept prismes de flint-glass le plus dense, qui me donne un angle de réfraction de plus de 300 degrés; que cette dispersion est encore augmentée par un autre prisme de flint-glass dense, de 60 degrés, et un prisme à vision directe, d'égal pouvoir; enfin, que je ne suis pas content encore de cette dispersion, qui est plus que double de celle qu'emploie le P. Secchi, et j'espère, dans quelques jours, avoir à ma disposition un pouvoir double de celui dont je dispose aujourd'hui."

Here is Father Secchi's rejoinder[1]:—

"ROME, *ce* 23 *août*, 1869.

"Je viens de recevoir le *Compte Rendu* du 16 août dans lequel est insérée la réponse de M. Lockyer à ma dernière Communication. Comme ce n'est pas moi qui ai commencé ce débat, je prie l'Académie de me permettre pour la dernière fois une petite réplique.

"1° M. Lockyer cherche à mettre en contradiction la couche à spectre continu avec ce que j'ai dit *que l'élargissement des raies constaté dans le noyau se reproduit près du bord.* M. Lockyer confond ici ce qui se voit à l'*intérieur* du disque dont je parle dans cette phrase, avec ce que j'ai dit dans une autre phrase du spectre continu qui reste à l'*extérieur* du disque. De cette manière la contradiction disparaît complétement (p. 453).

"2° Quant à mon autre assertion, qu'il prétend aussi contenir une contradiction, sur la largeur des raies qui, étant plus grande à la base, prouve l'*influence de la pression supérieure*, je prie M. Lockyer d'observer que je n'ai nié en aucune manière que la pression puisse produire ces effets, mais que j'ai dit simplement que l'irradiation et aussi l'intensité de la lumière pourraient bien y contribuer. Cette seconde contradiction ne subsiste donc pas plus que la première.

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[1] Vol. cit. p. 549, *et seq.*, August 30, 1869.

Observations of Magnesium.

"3° Je n'ai jamais jeté de doute sur la véracité de M. Lockyer comme il l'insinue, p. 454, lig. 18. Cela a toujours été loin de ma pensée. Mais j'ai dit que le renversement observé par moi était bien différent de celui qu'il a vu. J'ai vu (et je le maintiens, car l'observation a duré deux heures et je ne me suis pas trompé) renversée seulement une raie du magnésium, et j'ai constaté que l'intervalle entre les deux autres était devenu plus brillant. Cela explique, du reste, la *double* raie brillante vue dans l'éclipse par M. Rayet, qui en a vu *deux* et non *trois*. Cela n'empêche pas la vérité de l'assertion de M. Lockyer qui dit avoir vu les raies toute trois renversées. Mais cela serait une observation différente et qu'on ne peut confondre, ni par le fait ni par la date, avec la mienne.

"4° Je ne m'occuperai pas de ce qui regarde les théories ; parce que si moi-même j'ai essayé quelque chose, dans ce sens, je crois que cela peut bien se faire tout en admettant une insuffisance d'un côté et de l'autre. Mais pour ce qui regarde l'inexactitude qui ressortirait d'avoir affirmé que les masses suspendues dans l'atmosphère du Soleil sont le résultat des observations antérieures des éclipses, la chose est si bien connue, que je ne m'y arrêterai pas. M. Mathieu le premier et après lui un grand nombre d'observateurs ont constaté les arcs rosés outre les proéminences: or, ce n'est pas cela qui constitue ce que M. Lockyer a appelé *chromosphère*. Le nom sans doute appartient à M. Lockyer, mais la chose existait bien avant qu'il eût employé cette dénomination.

Mathieu's observations.

"5° M. Lockyer demande une démonstration de l'existence de ce milieu dans lequel peuvent nager ces masses d'hydrogène. Je lui répondrai qu'il n'en faut pas chercher une ailleurs que dans le fait de leurs formes définies ellesmêmes, et que cette atmosphère est bien sensible dans les éclipses à une distance bien plus grande que n'atteignent les proéminences et que nos photographies du *Desierto* nous ont signalé la forme elliptique de cette enveloppe plus relevée à l'équateur qu'aux pôles du globe solaire. Cette atmosphère peut bien contenir de l'hydrogène plus froid et d'autres gaz raréfiés, bien que l'hydrogène soit le plus léger des gaz (ce qui est bien connu), mais que par la diffusibilité propre à toutes les substances gazeuses il peut se mêler à d'autres d'un poids spécifique plus grand.

Place of Na and Mg vapours.

" 6° Je ne comprends pas ce que M. Lockyer dit relativement aux vapeurs de sodium et de magnésium placés *immédiatement au-dessous de la photosphère* (p. 456, lig. 5). Je ne sais pas comment on peut admettre la possibilité de constater ce *dessous* ; car la profondeur de la photosphère est insondable pour nous. Autrefois j'avais cru moi-même que la profondeur des taches était la mesure de l'épaisseur de la couche photosphérique ; mais cette théorie aujourd'hui n'est plus soutenable, et en cela je n'ai pas de difficulté à admettre que je suis maintenant en contradiction avec ce que j'ai avancé autrefois. Jamais je ne rougirai de m'instruire.

Bright lines in spots.

" 7° Finalement je vois que M. Lockyer avoue lui-même que les raies brillantes se voient beaucoup mieux à travers les taches. Ce fait est un peu atténue, mais c'est déjà beaucoup. Dans une prochaine Communication je donnerai les détails de mon Journal d'Observations sous ce rapport, et l'on verra que c'est précisément d'après cette différence considérable d'absorption élective qu'on doit juger de la constitution intime des matières qui remplissent les cavités des taches. Je n'omettrai pas en fin de dire que, comme dans les observations des bandes de Jupiter, par exemple, un trop fort grossissement nuit pour en saisir bien les nuances, il en est aussi de même dans celles qui nous occupent et dans lesquelles un grossissement et une dispersion trop forts, affaiblissant trop la lumière, peuvent être un obstacle pour reconnaître les différents degrés de lumière.

" Comme une grande partie de cette discussion roule sur

des inexactitudes provenant de ce que mes Communications ont été trop abrégées, je prierai l'Académie, dans une prochaine Communication, d'accepter quelques pages de la traduction littérale de mon Journal, dans lequel mes observations sont enregistrées avec les détails nécessaires pour éviter toute interprétation erronée."

Here I need scarcely say I let the discussion drop. It is consoling to know that all the points at issue have now been settled.

SIXTH PAPER.

Spectroscopic Observations of the Sun. No. VI.—By J. NORMAN LOCKYER, F.R.S. From the Proceedings of the Royal Society, No. 120, 1870. Received April 27, 1870.[1]

SIXTH PAPER.

THE weather lately has been fine enough and the sun high enough during my available observation time to enable me to resume work.

The crop of new facts is not very large, not so large as it would have been had I been working with a strip of the sun, say fifty miles or a hundred miles wide, instead of one considerably over a thousand—indeed, nearer two thousand—in width; but in addition to the new facts obtained, I have very largely strengthened my former observations, so that the many hours I have spent in watching phenomena, now perfectly familiar to me, have not been absolutely lost.

The negative results which Dr. Frankland and myself have obtained in our laboratory-work in the matter of the yellow bright line, near D, in the spectrum of the chromosphere being a hydrogen line, led me to make a special series of observations on that line, with a view of differentiating it, if possible, from the line C.

C *and* D_3 *differ in height.*

It had been remarked, some time ago, by Professor Zöllner, that the yellow line was often less high in a prominence than the C line; this, however, is no evidence (bearing in mind our results with regard to magnesium). The proofs I have now to lay before the Royal Society are of a different order, and are, I take it, conclusive.

D_3 *bright below chromosphere.*

I. With a tangential slit I have seen the yellow line bright below the chromosphere, while the C line has been dark; the two lines being in the same field of view.

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[1] *Proc. R.S.*, vol. xviii. p. 354.

II. In the case of a bright prominence over a spot *on the disc*, the C and F lines have been seen bright, while the yellow line has been invisible.

Motion in D$_3$ *less than in* C *and* F.

III. In a high-pressure injection of hydrogen the motion indicated by change of wave-length has been *less* in the case of the yellow line than in the case of C and F.

The pressure also less.

IV. In a similar quiescent injection the pressure indicated has been less.

D$_3$ *broken when* C *was whole.*

V. In one case the C line was seen long and unbroken, while the yellow line was equally long, but *broken*.

The circumstance that this line is so rarely seen dark upon the sun, makes me suspect a connection between it and the line at 5015 Ångström, which is also a bright line, and often is seen bright in the chromosphere, and then higher than the sodium and magnesium lines, when they are visible at the same time: and the question arises, must we not attribute these lines to a substance which exists at a higher temperature than those mixed with it, and to one of very great levity? for its absorption-line remains invisible, as a rule, in spot-spectra.

I have been able to make a series of observations on the fine spot which was visible when I commenced them on the 10th instant, not far from the centre of its path over the disc. At this time, the spot, as I judged by the almost entire absence of indications of general absorption in the penumbral regions, was shallow, and this has happened to many of the spots seen lately. A few hours' observation showed that it was getting deeper apparently, and that the umbræ were enlarging and increasing in number, as if a general downsinking were taking place; but clouds came over, and the observations were interrupted.

Bright prominence over umbra.

By the next day (April 11) the spot had certainly developed, and now there was a magnificently bright prominence, completely over the darkest mass of umbra, the prominence being fed from the penumbra or very close to it, a fact indicated by greater brilliancy than in the bright C and F lines.

SIXTH PAPER.

April 12. The prominence was persistent.

April 15. Spot nearing the limb, prominence still persistent over spot. At eleven I saw no prominence of importance on the limb, but about an hour afterwards I was absolutely startled by a prominence not, I think, depending upon the spot I have referred to, but certainly near it, more than 2′ high, showing a tremendous motion towards the eye. There were light clouds, which reflected to me the solar spectrum, and I therefore saw the black C line at the same time. The prominence C line (on which changes of wave-length are not so well visible as in the F line) was only coincident with the absorption-line for a few seconds of arc!

Change in the direction of the motion.

Ten minutes afterwards the thickness of the line towards the right was all the indication of motion I got. In another ten minutes the bright and dark lines were coincident.

And shortly afterwards what motion there was was towards the red.

I pointed out to the Royal Society, now more than a year ago,[1] that the largest prominences, ***as seen at any one time***, are not necessarily those in which either the intensest action or the most rapid change is going on. From the observations made on this and the following day, I think that we may divide prominences into two classes:—[2]

Two classes of prominence.

I. Those in which great action is going on, lower vapours being injected; in the majority of cases these are not high, they last only a short time—are throbs, and are oft renewed, and are not seen so frequently near the sun's poles as near the equator. They often accompany spots, but are not limited to them. These are the intensely bright prominences of the American photographs.

II. Those which are perfectly tranquil, so far as wave-length evidence goes. They are often high, are persistent, and not very bright. These do not, as a rule, accompany spots. These are the "radiance" and dull prominences shown in the American photographs.

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[1] *Proc. Roy. Soc.*, 1869, p. 354, March 17.

[2] See Note L.

I now return to my observations of the spot. On the 16th the last of the many umbræ was close to the limb, and the most violent action was indicated occasionally. I was working with the C line, and certainly never saw such rapid changes of wave-length before. The motion was chiefly horizontal, or nearly so, and this was probably the reason why, in spite of the great action, the prominences, three or four of which were shot out, never rose very high.

Mr. Holiday's drawings.

I append some drawings made, at my request, by an artist, Mr. Holiday, who happened to be with me, and who had never seen my instrument or the solar spectrum widely dispersed before. I attach great importance to them, as they are the untrained observations of a keen judge of form.

Bombs!

The appearances were at times extraordinary and new to me. The hydrogen shot out rapidly, scintillating as it went, and suddenly here and there the bright line, broad and badly defined, would be pierced, as it were, by a line of intensely brilliant light parallel to the length of the spectrum, and at times the whole prominence spectrum was built up of bright lines so arranged, *indicating that the prominence itself was built up of single discharges*, shot out from the region near the limb with a velocity sometimes amounting to 100 miles a second. After this had gone on for a time, the prominence mounted, and the cyclonic motion became evident; for away from the sun, as shown in my sketch, the separate masses were travelling away from the eye; then gradually a background of less luminous hydrogen was formed, moving with various velocities, and on this background the separate "bombs" appeared (I was working with a vertical spectrum) like exquisitely jewelled ear-rings.

It soon became evident that the region of the chromosphere just behind that in which the prominence arose,

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was being *driven back* with a velocity something like twenty miles a second, the back-rush being so local that with the small image I am unfortunately compelled to use, both the moving and rigid portions were included in the thickness of the slit. I saw the two absorption-lines overlap.

These observations were of great importance to me; for the rapid action enabled me to put together several phenomena I was perfectly familiar with separately, and see their connected meaning.

They may be summarized as follows, and it will be seen that they teach us much concerning the nature of prominences. When the air is perfectly tranquil in the neighbourhood of a large spot, or, indeed generally in any part of the disc, we see absorption-lines running along the whole length of the spectrum, crossing the Fraunhofer lines, and they vary in depth of shade and breadth according as we have pore, corrugation, or spot under the corresponding part of the slit—a pore, in fact, is a spot. Here and there, where the spectrum is brightest (where a bright point of facula is under the slit), we suddenly see an intensely bright lozenge of light. This I take to be due to bright hydrogen at a greater pressure than ordinary, and this then is the reason of the intensely bright points seen in ranges of faculæ observed near the limb.

A pore is a spot.

The appearance of this lozenge in the spectroscope, which (fig. 149) indicates a diminution of pressure round its central portion, is the signal for some, and often all of the following phenomena:—

Results of the appearance of a "lozenge."

I. A thinning and strange variations in the visibility and thickness of the hydrogen absorption-line under observation.

II. The appearance of other lozenges in the same locality.

III. The more or less decided formation of a bright prominence on the disc.

IV. If near the limb, this prominence may extend beyond it, and its motion-form will then become more easy of observation. In such cases the motion is cyclonic in the majority of cases, and generally very rapid, and—another feature of a solar storm—the photospheric vapours are torn up with the intensely bright hydrogen, *the number of bright lines visible determining the depth from which the vapours are torn*, and varying almost directly with the amount of motion indicated.

Here, then, we have, I think, the chain that connects the prominences with the brighter points of the faculæ.[1]

Connection of prominences with faculæ.

These lozenge-shaped appearances, which were observed close to the spot on the 16th, were accompanied by the "throbs" of the eruption, to which I have before referred. While Mr. Holiday was with me—a space of two hours—there were two outbursts, separated by a state of almost rest, and each outburst consisting of a series of discharges, as I have shown. I subsequently witnessed a third outburst. The phenomena observed in all three were the same in kind.

On this day I was so anxious to watch the various motion-forms of the hydrogen-lines, that I did not use the tangential slit. This I did the next day (the 17th of April) in the same region, when similar eruptions were visible, though the spot was no longer visible.

Reversal of the Fraunhofer lines at the base of the chromosphere.

Judge of my surprise and delight, when upon sweeping along the spectrum, I found HUNDREDS of the Fraunhofer lines beautifully bright at the base of the prominence !!!

The complication of the chromosphere spectrum was greatest in the regions more refrangible than C, from E to long past *b* and near F, and high-pressure iron vapour was one of the chief causes of the phenomenon.

I have before stated to the Royal Society that I have seen the chromosphere full of lines; but the fulness then was as emptiness compared with the observation to which I now refer.

A more convincing proof of the theory of the solar constitution, put forward by Dr. Frankland and myself, could

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[1] See Note M.

SIXTH PAPER.

scarcely have been furnished. This observation not only endorses all my former work in this direction, but it tends to show the shallowness of the region in which many of the more important solar phenomena take place, as well as its exact locality.

Appearance of F.

The appearance of the F line, with a tangential slit at the base of the prominence, included two of the lozenge-shaped, brilliant spots to which I have before referred: they were more elongated than usual—an effect of pressure, I hold; greater pressure and therefore greater complication of the chromosphere spectrum: this complication is almost impossible of observation on the disc.

It is noteworthy that, in another prominence, on the same side of the sun, although the action was great, the erupted materials were simple, *i.e.* only sodium and magnesium, and that a moderate alteration of wave-length in these vapours was obvious.

Telescopic appearance of the spots.

Besides these observations on the 17th, I also availed myself of the pureness of the air to telescopically examine the two spots on the disc, which the spectroscope reported tranquil as to up and down rushes. I saw every cloud-dome in their neighbourhood perfectly, and I saw these domes drawn out, by horizontal currents doubtless, in the penumbræ, while on the floors of the spots, here and there, were similar single cloud-masses, the distribution of which varied from time to time, the spectrum of these masses resembling that of their fellows on the general surface of the sun.

Spots are apparently shallowing.

I have before stated that the region of a spot comprised by the penumbra appears to be shallower in the spots I have observed lately (we are now near the maximum period of sun-spots): I have further to remark that I have evidence that the chromosphere is also shallower than it was in 1868.

I am now making special observations on these two points, as I consider that many important conclusions may be drawn from them.

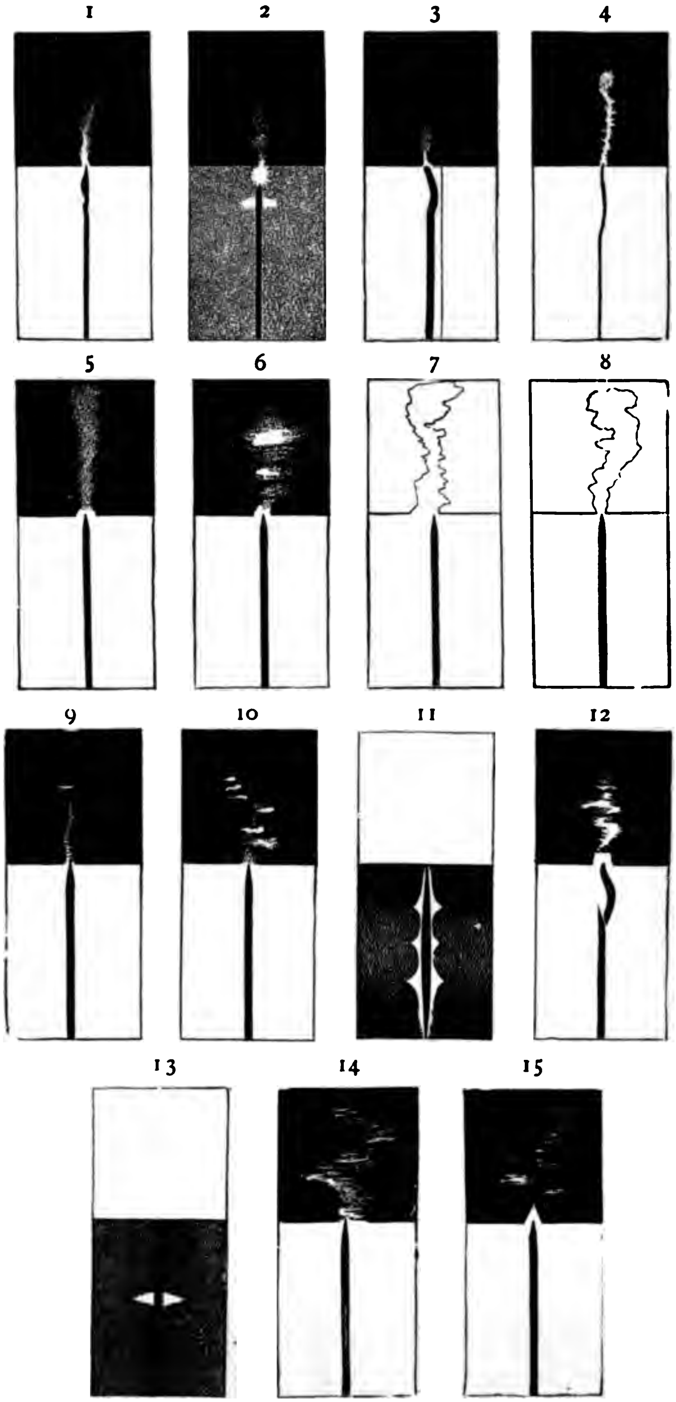

FIG. 149.—"Motion Forms," and "Lozenges." (See next page for description.)

FIFTH PAPER.

DESCRIPTION OF FIG. 149.

Mr. Holiday's drawings and remarks.

1. Prominence much bent.
2. Prominence encroaching over limb—bright line crossing black line.
3. Black line (F) curved downwards, sometimes nearly touching iron line below.
4.
5. Prominence nearly divided.
6. Intensely brilliant flashes above and below centres (of F lines); the interruptions very complete.

7 & 8. Curves in prominence very marked.

9, 10, 12, 14, 15. My own drawings, made during first and second outbursts.

11. A lozenge on the limb as seen with a tangential slit.

13. A lozenge as seen on the sun itself.

II.—*LABORATORY WORK.*

FIRST PAPER.

Preliminary note of Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun. By EDWARD FRANKLAND, F.R.S. and J. NORMAN LOCKYER, F.R.A.S. Received February 11, 1869.[1]

FIRST PAPER.

I. FOR some time past we have been engaged in a careful examination of the spectra of several gases and vapours under varying conditions of pressure and temperature, with a view to throw light upon the discoveries recently made bearing upon the physical constitution of the sun.

Although the investigations are by no means yet completed, we consider it desirable to lay at once before the Royal Society several broad conclusions at which we have already arrived.

Our start-points.

It will be recollected that one of us, in a recent communication to the Royal Society, pointed out the following facts:—

1. That there is a continuous envelope round the sun, and that in the spectrum of this envelope, which has been named for accuracy of description the "chromosphere," the hydrogen line in the green corresponding with Fraunhofer's line F, takes the form of an arrowhead, and widens from the upper to the lower surface of the chromosphere.

2. That ordinarily in a prominence the F line is nearly of the same thickness as the C line.

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[1] *Proc. R.S.*, vol. xvii. p. 288.

3. That sometimes in a prominence the F line is exceedingly brilliant, and widens out so as to present a bulbous appearance above the chromosphere.

4. That the F line in the chromosphere, and also the C line, extend on to the spectrum of the subjacent regions, and re-reverse the Fraunhofer lines.

5. That there is a line near D visible in the spectrum of the chromosphere to which there is no corresponding Fraunhofer line.

6. That there are many bright lines visible in the ordinary solar spectrum near the sun's edge.

7. That a new line sometimes makes its appearance in the chromosphere.

II. It became obviously, then, of primary importance—

1. To study the hydrogen spectrum very carefully under varying conditions, with the view of detecting whether or not there existed a line in the orange, and

2. To determine the cause to which the thickening of the F line is due.

D_3 *not in the* H *spectrum.*

We have altogether failed to detect any line in the hydrogen spectrum in the place indicated, *i.e.* near the line D; but we have not yet completed all the experiments we had proposed to ourselves.

With regard to the thickening of the F line we may remark that, in the paper by MM. Plücker and Hittorf, to which reference was made in the communication before alluded to, the phenomena of the expansion of the spectral lines of hydrogen are fully stated, but the cause of the phenomena is left undetermined.

Widening due to pressure.

We have convinced ourselves that this widening out is due to pressure, and not appreciably, if at all, to temperature *per se.*[1]

III. Having determined, then, that the phenomena presented by the F line were phenomena depending upon and indicating varying pressures, we were in a position to determine the atmospheric pressure operating in a prominence, in which the red and green lines are nearly of equal width, and in the chromosphere, through which the green line gradually expands as the sun is approached.[1]

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[1] See Note N.

Lowness of pressure in the chromosphere.

With regard to the higher prominences, we have ample evidence that the gaseous medium of which they are composed exists in a condition of excessive tenuity, and that at the lower surface of the chromosphere itself the pressure is very far below the pressure of the earth's atmosphere.

The bulbous appearance of the F line before referred to may be taken to indicate violent convective currents or local generations of heat, the condition of the chromosphere being doubtless one of the most intense action.

Hydrogen spectrum.

IV. We will now return for one moment to the hydrogen spectrum. We have already stated that certain proposed experiments have not been carried out. We have postponed them in consequence of a further consideration of the fact that the bright line near D has apparently no representative among the Fraunhofer lines. This fact implies that, assuming the line to be a hydrogen line, the selective absorption of the chromosphere is insufficient to reverse the spectrum. It is to be remembered that the stratum of incandescent gas which is pierced by the line of sight along the sun's limb, the radiation from which stratum gives us the spectrum of the chromosphere, is very great compared with the radial thickness of the chromosphere itself; it would amount to something under 200,000 miles close to the limb.

Although there is another possible explanation of the non-reversal of the D line, we reserve our remarks on the subject (with which the visibility of the prominences on the sun's disc is connected) until further experiments and observations have been made.

V. We believe that the determination of the above-mentioned facts leads us necessarily to several important modifications of the received theory of the physical constitution of our central luminary—the theory we owe to Kirchhoff, who based it upon his examination of the solar spectrum. According to this hypothesis, the photosphere itself is either solid or liquid, and it is surrounded by an atmosphere composed of gases and the vapours of the substances incandescent in the photosphere.

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[1] Will not this enable us ultimately to determine the temperature?

General simplicity of the chromosphere.

We find, however, instead of this compound atmosphere, one which gives us nearly or at all events mainly the spectrum of hydrogen; (it is not, however, composed necessarily of hydrogen alone; and this point is engaging our special attention;) and the tenuity of this incandescent atmosphere is such that it is extremely improbable that any considerable atmosphere such as the corona has been imagined to indicate, lies outside it,—a view strengthened by the fact that the chromospheric bright lines present no appearance of absorption, and that its physical conditions are not statical.

The photosphere probably gaseous or cloudy.

With regard to the photosphere itself, so far from being either a solid surface or a liquid ocean, that it is cloudy or gaseous or both follows both from our observations and experiments. The separate prior observations of both of us have shown :—

1. That a gaseous condition of the photosphere is quite consistent with its continuous spectrum. The possibility of this condition has also been suggested by Messrs. De la Rue, Stewart, and Loewy.

2. That the spectrum of the photosphere contains bright lines when the limb is observed, these bright lines indicating probably an outer shell of the photosphere of a gaseous nature.

A sun-spot a region of great absorption.

3. That a sun-spot is a region of greater absorption.

4. That occasionally photospheric matter appears to be injected into the chromosphere. May not these facts indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself or extremely near to it, instead of in an extensive outer absorbing atmosphere? And is not

this conclusion strengthened by the consideration that otherwise the newly-discovered bright lines in the solar spectrum itself should be themselves reversed on Kirchhoff's theory? This, however, is not the case. We do not forget that the selective radiation of the chromosphere does not necessarily indicate the whole of its possible selective absorption ; but our experiments lead us to believe that, were any considerable quantity of metallic vapours present, their bright spectra would not be entirely invisible in all strata of the chromosphere.

SECOND PAPER.

Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun, Stars, and Nebulæ.—Second Note. By E. FRANKLAND, F.R.S., and J. N. LOCKYER.—From the Proceedings of the Royal Society, No. 112, 1869.[1]

SECOND PAPER.

WE beg to lay before the Royal Society some further results of the researches in which we are engaged.

I. The Fraunhofer line in the solar spectrum, named *h* by Ångström, which is due to the absorption of hydrogen, is not visible in the tubes we employ with low battery and Leyden-jar power; it may be looked upon therefore as an indication of relatively high temperature. As the line in question has been reversed by one of us in the spectrum of the chromosphere, it follows that the chromosphere, when cool enough to absorb, is still of a relatively high temperature.

Hydrogen.

II. Under certain conditions of temperature and pressure, the very complicated spectrum[2] of hydrogen is reduced in our instrument *to one line in the green,*[3] corresponding to F in the solar spectrum.

Nitrogen.

III. The equally complicated spectrum of nitrogen is similarly reducible to one bright line in the green, with traces of other more refrangible faint lines.

IV. From a mixture of the two gases we have obtained a combination of the spectra in question, the relative brilliancy of the two bright green lines varying with the amount of each gas present in the mixture.

V. By removing the experimental tube a little further away from the slit of the spectroscope, the combined

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[1] *Proc. Roy. Soc.*, vol. xvii. p. 453.

[2] It has since been shown by Salet, Schuster and others that this complicated spectrum is due to impurities. Our attention was mainly confined to the undoubted lines of hydrogen. (1873.)

[3] See Note Q.

SECOND PAPER.

spectra referred to in II. and III. were reduced to the two bright lines.

VI. By reducing the temperature all spectroscopic evidence of the nitrogen vanished; and by increasing it, many new nitrogen lines make their appearance, the hydrogen line always remaining visible.

Bearing on the nebulæ.

The bearing of these latter observations on those made on the nebulæ by Mr. Huggins, Father Secchi, and Lord Rosse is at once obvious. The visibility of a single line of nitrogen has been taken by Mr. Huggins to indicate possibly, first, "a form of matter more elementary than nitrogen, and which our analysis has not yet enabled us to detect,"[1] and then secondly, "a power of extinction existing in cosmical space."[2]

Our experiments on the gases themselves show not only that such assumptions are unnecessary, but that spectrum analysis here presents us with a means of largely increasing our knowledge of the physical constitution of these heavenly bodies.

Already we can gather that the temperature of the nebulæ is lower than that of our sun, and that their tenuity is excessive; it is also a question whether the continuous spectrum observed in some cases may not be due to gaseous compression.

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[1] *Phil. Trans.* 1864, p. 444.

[2] Ibid. 1868, p. 544.

THIRD PAPER.

Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun, Stars, and Nebulæ.—Third Note. By E. FRANKLAND, F.R.S., and J. NORMAN LOCKYER, F.R.S.—From the Proceedings of the Royal Society, No. 115, 1869.[1]

THIRD PAPER.

I. IT has been pointed out by one of us that the vapours of magnesium, iron, &c. are sometimes injected into the sun's chromosphere, and are then rendered sensible by their bright spectral lines.[2]

II. It has also been shown (1) that these vapours, for the most part, attain only a very low elevation in the chromosphere, and (2) that on rare occasions the magnesium vapour is observed like a cloud separated from the photosphere.

The magnesium lines of unequal length.

III. It was further established on the 14th of March 1869, and a drawing was sent to the Royal Society indicating, that when the magnesium vapour is thus injected the spectral lines do not all attain the same height.

Thus of the b lines, b^1 and b^2 are of nearly equal height, but b^4 is much shorter.

IV. It has since been discovered that of the 450 iron lines observed by Ångström, only a very few are indicated in the spectrum of the chromosphere when iron vapour is injected into it.

V. Our experiments on hydrogen and nitrogen enabled us at once to connect these phenomena, always assuming, as required by our hypothesis,[3] that the great bulk of the absorption to which the Fraunhofer lines are due takes place in the photosphere itself.

It was only necessary, in fact, to assume that, as in the case of hydrogen and nitrogen, the spectrum became

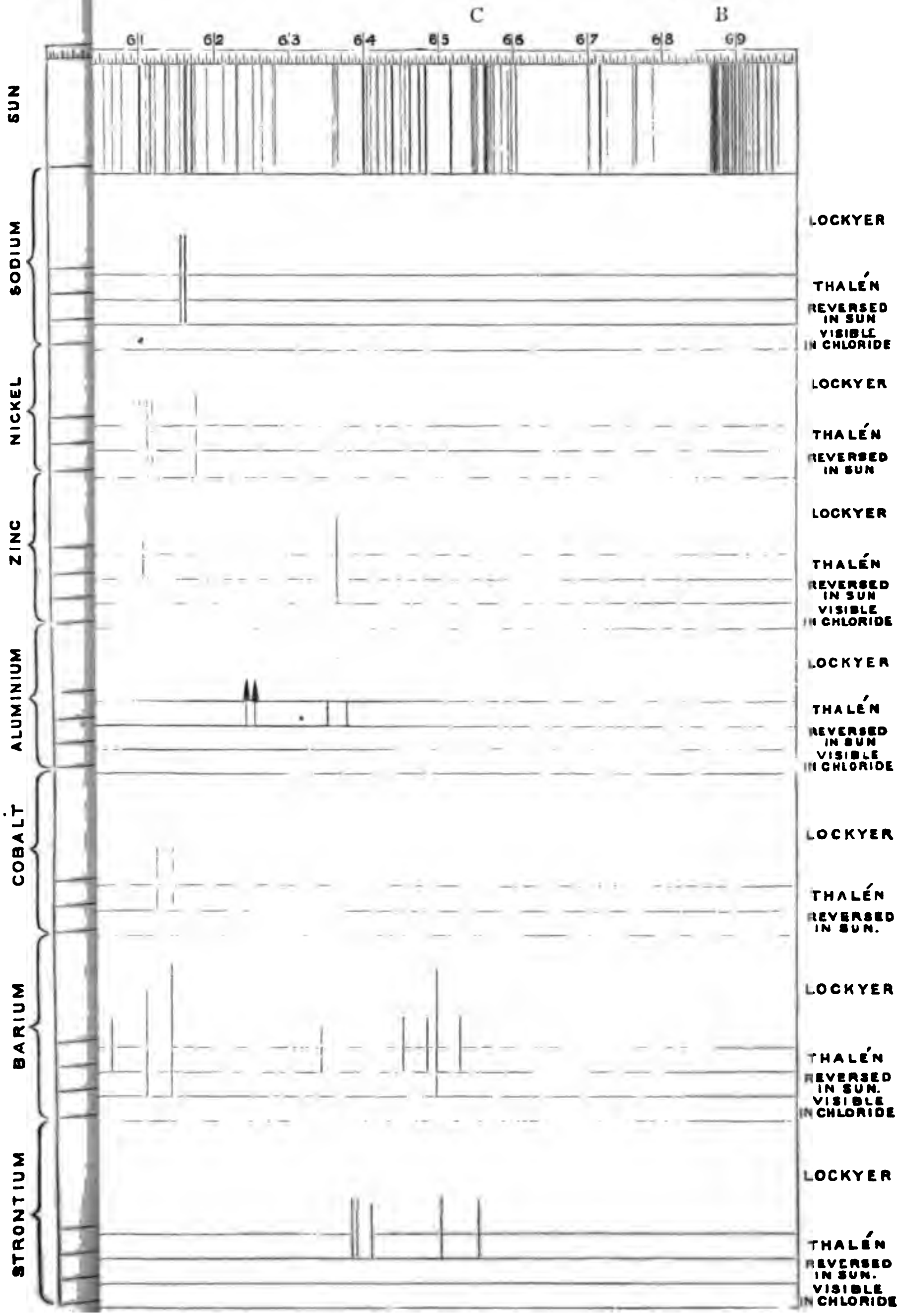
[1] *Proc. Roy. Soc.*, vol. xviii. p. 79.

[2] Ibid. vol. xvii. p. 351.

[3] Ibid. p. 290, and *ante*, p. 528.

VERSED IN THE SUN AND VISIBLE

Pl. V



simpler where the density and temperature were less, to account at once for the reduction in the number of lines visible in those regions where, on our theory, the pressure and temperature of the absorbing vapours of the sun are at their minimum.

VI. It became important, therefore, to test the truth of this assumption by some laboratory experiments, the preliminary results of which we beg to communicate in this Note, reserving details, and an account of the further experiments we have already commenced, for another paper under the above title.

We took the spark in air between two magnesium poles, so separated that the magnesium spectrum did not extend from pole to pole, but was visible only for a little distance, indicated by the atmosphere of magnesium vapour round each pole.

b behaves in the laboratory as it does in the sun.

We then carefully examined the disappearance of the *b* lines, *and found that they behaved exactly as they do on the sun.* Of the three lines the most refrangible was the shortest; and shorter than this were other lines, *which one of us has not yet detected in the spectrum of the chromosphere.*

This preliminary experiment, therefore, quite justified our assumption, and must be regarded as strengthening the theory on which the assumption was based—namely, that the bulk of the absorption takes place in the photosphere, and that it and the chromosphere form the true atmosphere of the sun. In fact, had the experiment been made in hydrogen instead of in air, the phenomena indicated by the telescope would have been almost perfectly reproduced; for each increase in the temperature of the spark caused the magnesium vapour to extend further from the pole, and where the lines disappeared a band was observed surmounting them, which is possibly connected with one which at times is observed in the spectrum of the chromosphere itself when the magnesium lines are not visible.

FOURTH PAPER.

Researches in Spectrum Analysis in connection with the Spectrum of the Sun. By J. NORMAN LOCKYER, F.R.S.

Received November 6,—read December 12, 1872.[1]

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THE researches, of which an account is given in the present communication, have been undertaken in continuation of those carried on by Dr. Frankland and myself at the Royal College of Chemistry, from which we arrived at the conclusion that the thickening of spectral lines was due to pressure, and not to temperature *per se.*[2] In our joint communications we pointed out that this held good for hydrogen in Geissler's tubes and for magnesium vapour,[3] when the spark was taken in air, by means of a method which enabled us to spectroscopically examine its various portions.

The magnesium experiment was important not only so far as the decrease of thickness of lines with decrease of density was concerned, but because it showed that one of the well-known triple lines in the spectrum of magnesium absolutely vanished altogether from the spectrum at some distance from the source of the supply of the vapour—that is, the pole of metallic magnesium. This result we also obtained, as stated in our note, when we observed the spectrum of the spark between two magnesium poles enclosed in a Geissler's tube in an atmosphere of hydrogen in which the pressure of the gas was gradually reduced.

Experiments with sodium vapour.

In some experiments with sodium vapour which were not referred to in the papers in question,[4] Dr. Frankland and myself observed the same phenomena. The experiments were conducted as follows:—

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[1] *Philosophical Transactions*, 1873.

[2] *Proceedings of the Royal Society*, vol. xvii. p. 289, *ante*, p. 526.

[3] Ibid. vol. xviii. p. 79, *ante*, p. 532.

[4] This experiment was first exhibited at a Lecture given by me at the Royal Institution in May, 1869.

Method of making the experiment.

(1.) Into a piece of hard glass combustion-tube, thoroughly cleaned and closed at one end, a few pieces of metallic sodium, clean and as free as possible from naphtha, were introduced. The end of the tube was then drawn out and connected with a Sprengel pump and exhausted as rapidly as possible. Hydrogen was then admitted, and the tube re-exhausted, and, when the pressure was again reduced to a few millimetres, carefully sealed up. The tube thus prepared was placed between the slit plate of a spectroscope and a source of light giving a continuous spectrum.

Generally, unless the atmosphere of the laboratory was very still and free from dust, the two bright D lines could be distinctly seen on the background of the bright continuous spectrum.

The D lines reversed.

The tube containing the sodium was then heated with a Bunsen flame, and the spectrum carefully watched. Soon after the application of the heat, a dark line, thin and delicate as a spider's thread, was observed to be slowly creeping down each of the bright sodium lines and exactly occupying the centre of each. Next, this thin black line was observed to thicken at the *top* where the spectrum of the *lower* denser vapours was observed, and to advance downwards along the D line, until arriving at the bottom they both became black throughout; and if now the heat was still applied, thus increasing the density of the various layers of the sodium vapour, the lines began to broaden until, in spite of considerable dispersion, the two lines blended into one. The source of heat being now removed, the same changes occurred in inverse order; the broad band split into two lines, gradually the black thread alone was left, and finally that vanished, and the two bright lines were restored.

(2.) This experiment was then varied in the following way:—Some pieces of metallic sodium were introduced into a test-tube, and a long glass tube conveying coal-gas passed to the bottom, an exit for the gas being also provided at the top. The sodium was now heated and the

The black lines thin out when the vapour is diluted.

flow of coal-gas stopped. In a short time the reversal of the D lines was complete. The gas was now admitted, and a small quantity only had passed when the black lines were reduced to threads.

In my former communications to the Royal Society I have pointed out the extreme importance of these facts in connection with solar and stellar physics. In observing the sun by the new method, we get various Fraunhofer lines thickened in the spots and thinned in the chromosphere and prominences; and in these latter, in some instances, notably in the case of F, we find the lines gradually widening as they approach the limb of the sun.

While this may be remarked as a solar demonstration of the correctness of the conclusion at which Dr. Frankland and myself had arrived, it is to be noted that bright line prominences may occasionally be seen on the sun's disc over or near spots in the spectrum of which the same lines are thick, while this phenomenon could not exist if the thickening of the lines were due to temperature alone.

METHOD EMPLOYED.[1]

The method of observing spectra to which I have already referred, and which has been adopted in the work of which I now propose to give an account, consists in throwing an image of the spark on the slit of a spectroscope in the laboratory experiments in exactly the same manner in which I proposed, in 1866, that an image of the sun should be thrown on the slit in order to spectroscopically examine minute portions of the sun and his surrounding atmosphere.

A section of the spark is observed.

It is obvious that in this method the image of the slit will be associated in the spectroscope with an image of a section of the spark; and that if from any cause there be various shells of vapour surrounding each pole, which shells give different spectra, then these spectra will be sorted out so that their variations may be traced from pole to pole.

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[1] This method was first exhibited at a lecture at the Royal Institution April 2nd, 1870. The same method has more recently been employed with great success by M. Salet in a research on the spectra of the Metalloids.

The arrangements adopted will be easily gathered from the annexed woodcut (Fig. 150) and the accompanying

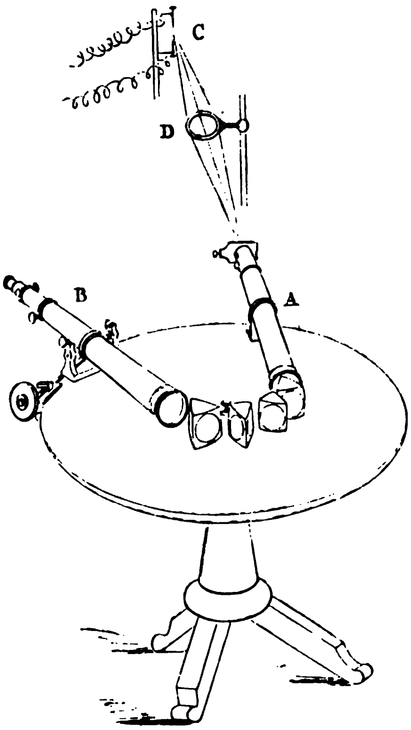

FIG. 150.—A, collimator ; B, observing-telescope ; C, spark ; D, lens.

description. It is scarcely necessary to add that an important condition of this new method is that the object-glass of the collimator should be filled with light, and also that no light should be wasted. So long as these conditions obtain, conjugate foci and different lenses may be employed

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and the size of the image varied at pleasure, and still the brightness of the spectrum will be sufficient.

Spectroscope.

The instruments with which the observations have been made are as follows:--A large spectroscope, a sister instrument to that used by Bunsen and Kirchhoff in their celebrated researches, and made by the same maker, Steinheil, of Munich.[1] It is furnished with four prisms of flint glass. Three are of an angle of 45° and one of 60°. The general arrangements of the instruments are described by Kirchhoff in his memoir.

In front of the slit plate is placed a lens throwing on the slit the image of the spark.

Coil.

A coil, made by Apps, and giving a 4-inch spark.

A large Leyden jar has also been occasionally used as a condenser on the secondary wire.

Beneath the observing-telescope is placed a commutator, by which the current is controlled by the observer without changing his position.

The window of my laboratory looks due south, and the collimator is placed in the same direction; and when it became necessary to have the solar spectrum in the field, the light reflected from a heliostat placed outside the laboratory in direct prolongation of the line of collimation was thrown on to the lens and thus on to the slit, where the size and intensity of the images could be varied at pleasure by altering the position of the lens.

Photographic arrangements.

When it was required to photograph a spectrum, the ordinary observing-telescope of the spectroscope was dismounted, and its place supplied by a telescope of 3¾ inches aperture and 49 in. focus. This was supported on the cast-iron table of the spectroscope at one end, and at the other on a stand. The eyepiece and its mounting were removed, and against the end of the tube, thus left free, a small camera-box holding a plate 4¼ in. by 3¼ in.

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[1] This spectroscope has been temporarily placed at my disposal by Professor Guthrie, of the Royal School of Mines, to whom my best thanks are due.

was placed, and the photograph taken in the usual manner, the focus being obtained partly by careful observation with powerful magnifiers, and partly by trial-plates.

From the time of Wheatstone's first experiments, when in 1835 he stated that if the poles consisted of two different metals the spectrum contained the lines of both metals, down to the researches of Stokes, Miller and Robinson in 1862, there is no reference, so far as I can find, to any localization of light in any portion of the *breadth* of the spectrum. In the case of the spark taken between two poles, *e.g.*, in air, the spectrum is generally one in which the lines of the two vapours and of air are blended together, all the lines running across the field.

Stokes's observations.

But under certain conditions this is not so. Thus Stokes,[1] who used the spark itself instead of a slit, remarked that the metallic lines are "distinguished from air-lines by being formed only at an almost insensible distance from the tips of the electrodes, whereas air-lines would extend right across."

Miller.

Miller,[2] who used a slit and a spark close to it, referring to his photographs of electric spectra, remarks:—"The marginal extremities of the metallic lines leave a stronger image than their central portions," and the extremities of these interrupted lines he terms "dots."

Robinson.

On the same subject Robinson[3] writes:—"At that boundary of the spectrum which corresponds to the negative electrode (and in a much less degree at the positive extremity) intense lines are seen, . . . which, however, are short."

Thalén.

Thalén (though he also did not adopt the method used by Dr. Frankland and myself in and since 1869) observed this localization to a certain extent, doubtless on account of the long collimator which he employed.

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[1] *Philosophical Transactions*, vol. clii. 1862, p. 603.

[2] Op. cit., p. 877.

[3] Op. cit., p. 947.

Thalén's remarks.

He remarks[1] :—" Il y a aussi des raies brillantes qu'on n'observe que dans des cas exceptionnels, comme, par exemple, quand la quantité de la substance, soumise à l'expérience, est très-abondante ou quand l'incandescence devient très-vive. Ces raies qui se présentent ordinairement aux bords du spectre sous la forme de points d'aiguille, même quand les autres raies du métal forment des lignes continues en travers du spectre, ont été représentées sur la planche par des lignes très-courtes."

Before I proceed further I beg to refer to the two annexed woodcuts (Figs. 151, 152), copied from photographs of a part of the spectrum observed when the jar-spark passes (1) between the poles of *zinc* and *cadmium*, and (2) between *cadmium* and *lead*, and the image is thrown on the slit. It will be seen that in the case of these metallic vapours (and it is true of all others that I have yet observed), the lines, as in the before-mentioned case of the triple line (*b*) of magnesium, are of unequal length, and that in the new method of observation the lines in the spectra of the two metallic vapours and of the air are separated in the clearest and most convenient manner, the air-lines going right across, and the lines of the metallic vapours extending to greater or less distances from each pole, and in some cases (*i.e.* of the longest lines) overlapping.

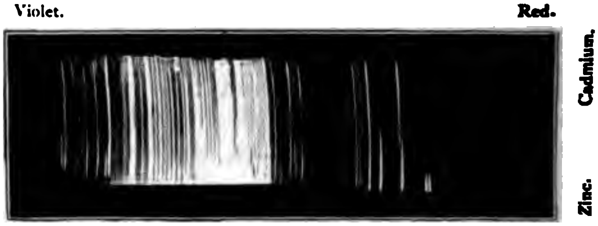

FIG. 151.—Long and short lines of Zinc and Cadmium.

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[1] " Mémoire sur la détermination des longueurs d'onde des raies métalliques," p. 12, printed in the Nova Acta Regiæ Societatis Scientiarum Upsaliensis, ser. iii. vol. vi. Upsala, 1868.

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Elements mapped.

With this communication are maps (Plates XI. XII. XIII.) of the spectra of the following elements made on this method, the jar being used :—Na, Li, Mg, Al, Mn, Co, Ni, Zn, Sr, Cd, Sn, Sb, Ba, and Pb. The lines were laid down from Thalén's maps, given in the memoir quoted above and on the same scale—namely, 2 centimetres to each $\frac{1}{100000}$ millim. of wave-length. The spectra were then carefully and repeatedly observed, and the comparative lengths of the lines estimated and laid down over their respective wave-lengths.

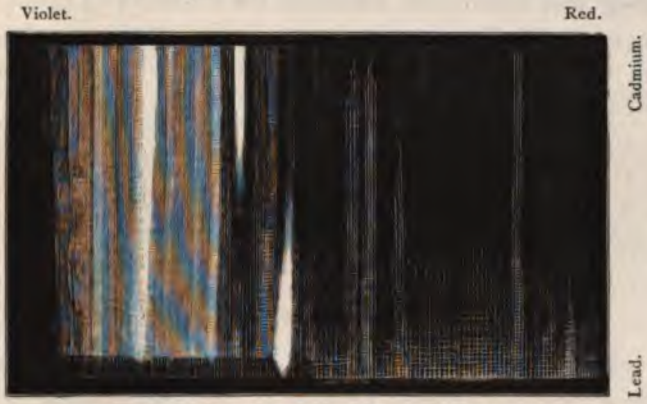

FIG. 152.—Long and short lines of Lead and Cadmium.

Behaviour of the longest lines.

At the same time that these spectra have been mapped with the spark taken in air, many of them have also been observed when their metals were enclosed in tubes and subjected to a continually decreasing pressure, as in the case of the before-mentioned experiment with magnesium. *In all these experiments it was found that the longest lines invariably remained visible longest.*

In the case of zinc the effect of these circumstances was very marked, and they may be given as a sample of the phenomena generally observed. When the pressure-gauge connected with the Sprengel pump stood at from 35 to 40 millimetres, the spectrum at the part observed was normal, *Observations on zinc.*

except that the two lines 4924 and 4911[1] (both of which, when the spectrum is observed under the normal pressure, are lines with thick-wings) were considerably reduced in width. On the pump being started these lines rapidly decreased in length, as did the line at 4679; 4810 and 4721 being almost unaffected; at last the two at 4924 and 4911 vanished, as did 4679, and appeared only at intervals as spots on the poles, the two 4810 and 4721 remaining little changed in length, though much in brilliancy. This experiment was repeated four times, and on each occasion the gauge was found to be almost at the same point, viz.—

1st observation, when the lines 4924 and 4911 were gone the gauge stood at	. . . .	30 mm.
2nd do.	. . . .	29 „
3rd do.	. . . .	29 „
4th do.	. . . .	31 „

A rise to 34 millimetres was sufficient to restore the lost lines.

Experiments with Chemical Compounds.

Since it appeared that the purest and densest vapour alone gave the greatest number of lines, or, in other words, that the truly complete spectrum of a body is alone to be obtained upon the metallic pole itself where the vapour is densest and purest, it became of interest to examine the spectrum of a compound consisting of a metal combined with a non-metallic element.

To this end a number of experiments was made, in which the metallic spectra were compared with those given by the same metals when combined with chlorine under the same conditions as in the former experiments.

Spectra of the chlorides observed.

The compounds thus experimented on were as follows, the jar being used:—$Li\,Cl$, $Na\,Cl$, $Mg\,Cl_2$, $Zn\,Cl_2$, $Sr\,Cl_2$, $Cd\,Cl_2$, $Ba\,Cl_2$, $Pb\,Cl_2$, and $Al_2\,Cl_6$. It was found in all

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[1] Thalén's scale as given by Watts.

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Effect of combination with chlorine.

cases that the difference between the spectrum of the chloride and the spectrum of the metal was:—*That under the same spark conditions the short lines were obliterated, while the air-lines remained unchanged in thickness.*

Changing the spark-conditions by throwing the jar out of the circuit, this change was shown in its strongest form, the final results being that only the very longest lines in the spectrum of the metal remained.

The following are the details of the experiments made under these conditions:—

Apparatus for observing the spectra of dry salts.

Method of Observation.—Some pieces of stout aluminium wire, 10 millims. long and 3 millims. in diameter, were taken; one end was flattened for about one-third of the length for the purpose of inserting it in the spark-holder, and the other was drilled down in the direction of the axis for from 2 to 3 millims., and thus formed into a small conical cup; a very fine hole was then drilled through the side of this cup at the bottom, and the flattened end carefully split. Through the lateral hole a piece of platinum wire, 0·5 millim. in diameter, was passed, and one end brought round through the split end of the aluminium, while the other was brought up the centre of the cup. The split was now closed by strong pressure in a vice, and the ends of the platinum wire cut off. The whole now presented the appearance of a small candle, the platinum wire representing the wick: the accompanying figures (Figs. 153—154) will render

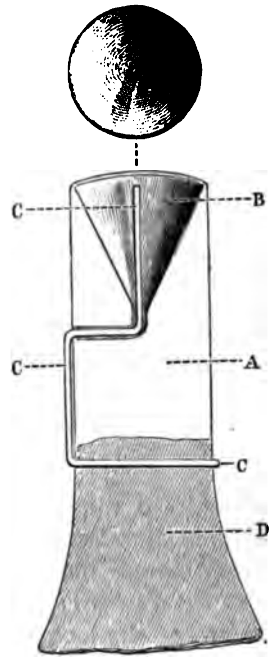

FIG. 153.—Plan and section of cup used with salts. A, aluminium wire; B, cup-shaped cavity drilled in it; C, platinum wire; D, flattened and split portion of the aluminium wire.

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Method of use.

the preceding statement clear.[1] Round this wick the chloride in fine powder was tightly rammed down. [A similar cup, without the wick, was used for the examination of the spectra of metallic barium, strontium and lithium, the metal being hammered into it.] One of these cups with the chloride replaced the lower pole in the spark-holder, the upper one being composed of copper, that metal being selected as being a good conductor, and giving a very simple and easily recognized spectrum.

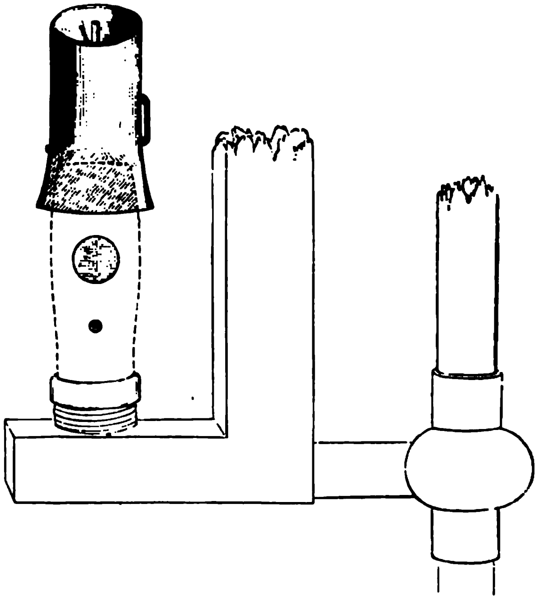

FIG. 154.—Aluminium cup placed in the spark-stand as in use.

Lithic chloride.

Chlorides observed.—Lithic Chloride, Li Cl. The wave-lengths of the longest lines of this metal are as follows:—6705·2, 6102·0, and 4602·7, the latter (in the blue) being a wide and winged line. When the spectrum of the chloride is observed, the red line, 6705·2, is seen right across the spectrum; the orange, 6102·0, is faintly visible for about half the distance; 4602·7 has vanished altogether. In the case of lithium this extinction can be carried further in the flame reaction with an ordinary Bunsen burner in which the red line 6705·2 is alone seen.[1]

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[1] The object of the wick was to confine the spark to the centre of the dry chloride. Before it was adopted the spark was very unsteady, leaping about from side to side of the cup.

Sodic chloride.

Sodic Chloride, Na Cl.—The D line $\left.\begin{matrix}5895{\cdot}0\\5889{\cdot}0\end{matrix}\right\}$ is by very far the longest line in the sodium spectrum; it is, in fact, the longest metallic line I have observed. After D come $\left.\begin{matrix}6160{\cdot}0\\6154{\cdot}2\end{matrix}\right\}$ in the red and $\left.\begin{matrix}5687{\cdot}2\\5681{\cdot}4\end{matrix}\right\}$ in the yellow, the latter pair having a slight advantage over the former. $\left.\begin{matrix}5154{\cdot}8\\5152{\cdot}5\end{matrix}\right\}$ come next, and the shortest is 4982·5, really a double line, but so nebulous and ill-defined that Thalén has represented it as single. In the chloride we find D $\left.\begin{matrix}5895{\cdot}0\\5889{\cdot}0\end{matrix}\right\}$ all across the spectrum, and all the others have vanished but a trace of $\left.\begin{matrix}5687{\cdot}2\\5681{\cdot}4\end{matrix}\right\}$.

Magnesic chloride.

Magnesic Chloride, $MgCl_2$.—Magnesium has three lines (*b*) surpassing all the others in length; their wave-lengths are 5183·0, 5172·0, and 5166·7; these lines alone are constant in the chloride; 4481·0, the winged line, sometimes flashes in.

Zincic chloride.

Zincic Chloride, $Zn\,Cl_2$.—Zinc has three long lines in the blue, 4809·7, 4721·4, 4679·5; these only are visible in the spectrum of the chloride. One line, 6362·5, in the extreme orange, is of the same length apparently as the shortest of the three blue lines, but is not visible, possibly on account of its faintness.

Strontic chloride.

Strontic Chloride, $Sr\,Cl_2$.—Strontium has one extremely long line, 4607·5, and this with two in the indigo, 4226·3

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[1] It is necessary in dealing with Li Cl and Na Cl to have the poles rather far apart (8 to 10 millims.), as, on account of the easy volatility of these chlorides, if the poles are close all the lines appear stretching across the spectrum.

FOURTH PAPER.

and 4215·3 next in length to it, are alone seen in the chloride spectrum.

Cadmic chloride.

Cadmic Chloride, Cd Cl_2.—Cadmium, like zinc, has three lines of greater length than all the rest, one in the blue-green, 5085·0, and two, 4799·0 and 4676·8, in the blue. These alone appear in the spectrum of the chloride.

Plumbic chloride.

Plumbic Chloride, Pb Cl_2.—Lead has its longest line, 4058·0, in the violet; this line alone is visible in the chloride spectrum; 5607·0 in the yellow-green, which appears nearly as long, is not visible.

Baric chloride.

Baric Chloride, Ba Cl_2.—Barium has three lines distinguished by their great length; they are 5534·5 in the yellow-green, 4933·4 in the green, and 4553·4 in the indigo. These only are visible in the chloride spectrum.

Aluminic chloride.

Aluminic Chloride, Al_2Cl_6.—Aluminium has but two long lines, which fall between H_1 and H_2, and are of the following wave-lengths, 3961·0 and 3943·0; these alone are visible in the spectrum of aluminic chloride.

Difference between elements of high and low atomic weights.

It will be seen from the foregoing that in the case of elements with low atomic weights, combined with one equivalent of chlorine, the number of lines which remain in the chloride is large—over 60 per cent. for instance in the case of Li, and 40 per cent. in that of Na.

While, on the other hand, in the case of elements with greater atomic weights, combined with two equivalents of chlorine, we get a much smaller number of lines remaining—8 per cent. for instance in the case of Ba. and 3 per cent. in the case of Pb.

Preliminary Experiments with Mechanical Mixtures.

Another series of experiments has had for its object the examination of the spectrum of mechanically mixed metals,—alloys prepared *ad hoc.* These experiments, which at present are preliminary only, were made because it seemed clear that the same law that was observed with the chlorides should hold good.

Effect of alloying.

A cursory examination of the spectra of some amalgams of tin and magnesium has shown that this is the case.

For instance, it is possible to begin with an alloy which shall only give us the longest line or lines in the spectrum of the smallest constituent, and by increasing the quantity of this constituent the other lines can be introduced in the order of their length. This reaction is so delicate that I learnt from it, a thing I had not before observed, that the least refrangible line of *b*, the triple line of magnesium, is really a little longer than its more refrangible companion; for the spectrum of magnesium was reduced to this one line in an alloy in which special precautions had been taken to introduce the minimum of magnesium.

Probable quantitative application.

It follows from this statement that not only is the spectrum analysis almost infinitely more delicate than it has hitherto been supposed to be in the case of the elements in which the difference between the longest and shortest lines is least,[1] but that in time it may become quantitative; for if the admixture of certain other bodies extinguishes the shorter lines of metallic spectra, it would seem that a series of carefully executed maps of the spectra of alloys, the proportions of the constituents of which are known, will place in our hands the means of determining (roughly it is true), by mere inspection, the quantity of the sought metal present in an alloy, the composition of which *quâ* that metal is unknown. At the same time it is clear that further progress must be made before such a method can be practically employed in the arts.

Although the working hypothesis which has suggested the various lines of research which have been followed is, I think, sufficiently clear, I refrain from dwelling upon it until other researches now in progress enable me more fully to judge of its value, and to state at greater length the various conclusions which may be drawn from it.

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[1] The great lengths of the lines of sodium, lithium, &c., at once account for the delicacy of their spectrum reactions.

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Application of these Observations to the Solar Spectrum

These observations have an important bearing upon t
solar spectrum, for the reason that, as is well known,
the lines known to exist in the spectrum of an eleme
supposed to be present in the sun's atmosphere are not
all cases reversed.

Before I proceed to give the facts in detail, it will be w
to go over the prior work of Kirchhoff and Ångströ
to see precisely the evidence on which our present kno
·ledge of the elements in the solar atmosphere, as det
mined by Kirchhoff's method of solar observation—that
the non-localization, or integration, of the various sol
regions, such as spots, faculæ and chromosphere—rests.

Kirchhoff's work.

Kirchhoff, in his paper referring to Fraunhofer's pri
determination of the double line D being coincident wi
a double line observed in the spectrum of sodium vapo
locates sodium vapour in the solar atmosphere, as Profess
Stokes had done before him. Coincident with all t
bright iron lines which he observed with the spark he us
(he only saw a small number of the lines), he found we
defined Fraunhofer lines. He therefore located iron vapo
in the atmosphere. The rest of the evidence relating
other metals I give from the translation of his memoir
Professor Roscoe.[1]

"As soon as the presence of one terrestrial element
the solar atmosphere was thus determined, and thereby t
existence of a large number of Fraunhofer lines explaine
it seemed reasonable to suppose that other terrestrial bodi
occur there, and that by exerting their absorptive pow
they may cause the production of other Fraunhofer line
for it is very probable that elementary bodies which occ
in large quantities on the earth, and are likewise disti
guished by special bright lines in their spectra, will, li
iron, be visible in the solar atmosphere. This is found

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[1] *Transactions of Berlin Academy*, 1861. Translated by Rosc
(Macmillan.)

be the case with calcium, magnesium, and sodium. The number of the bright lines in the spectrum of each of these metals is indeed small; but those lines, as well as the dark ones in the solar spectrum with which they coincide, are so uncommonly distinct that the coincidence can be observed with very great accuracy.

Kirchhoff's remarks.

"In addition to this, the circumstance that these lines occur in groups renders the observation of the coincidence of these spectra more exact than is the case with those composed of single lines. The lines produced by chromium also form a very characteristic group, which likewise coincides with a remarkable group of Fraunhofer lines; hence I believe that I am justified in affirming the presence of chromium in the solar atmosphere. It appeared of great interest to determine whether the solar atmosphere contains nickel and cobalt, elements which invariably accompany iron in meteoric masses. The spectra of these metals, like that of iron, are distinguished by the large number of their lines; but the lines of nickel, and still more those of cobalt, are much less bright than the iron lines, and I was therefore unable to observe their position with the same accuracy with which I determined the position of the iron lines. All the *brighter lines* of nickel appear to coincide with dark solar lines; the same was observed with respect to some of the cobalt lines,[1] *but was not seen to be the case with other equally bright lines of this metal.* From my observations I consider that I am entitled to conclude that nickel is visible in the solar atmosphere; I do not, however, yet express an opinion as to the presence of cobalt. Barium, copper, and zinc appear to be present in the solar atmosphere, but only in small quantities; the brightest of the lines of these metals correspond to distinct lines in the solar spectrum, but the weaker lines are not noticeable. The remaining metals which I have examined—viz. gold, silver, mercury, aluminium, cadmium, tin, lead, antimony, arsenic, strontium, and lithium—are, according to my observations, not visible in the solar atmosphere. Through the kindness of M. Grandeau, of Paris, I obtained several pieces of fused silicium; I was thus enabled, by using them as electrodes, to examine the spectrum of this element. The lines in the silicium spectrum are, however, with the exception of two broad green bands at 1810 and 1830, so deficient in luminosity that I was unable to determine their position with sufficient accuracy to reproduce them in my drawing. The two bright green bands do not correspond to dark bands in the solar spectrum, so that, as far as I have been able to determine, silicium is not visible in the solar atmosphere."

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[1] The italics are mine.

Results of Kirchhoff's work.

It will be seen from the foregoing that Kirchhoff dealt mainly with the brightest lines, although the test failed him in the case of cobalt, for a reason I shall show further on. Hence, as a result of Kirchhoft's work, we have in the solar atmosphere:—

Present.	*Doubtful.*	*Absent.*
Sodium.	Cobalt.	Gold.
Iron.		Silver.
Calcium.		Mercury.
Magnesium.		Aluminium.
Nickel.		Cadmium.
Barium.		Tin.
Copper.		Lead.
Zinc.		Antimony.
		Arsenic.
		Strontium.
		Lithium.
		Silicium.

Ångström and Thalén.

Ångström[1] gives no list such as this, but in its place a table of coincidences observed. Thalén, his associate, in a separate memoir[2] gives, however, as present in the sun:—

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[1] "Recherches sur le spectre solaire," par A. J. Ångström. *Spectre normal du Soleil.* Berlin, 1869.

[2] "Longueurs d'onde des raies métalliques," p. 11. *Nova Acta.* Upsala, 1868.

Sodium.
Iron.
Calcium.
Magnesium.
Chromium.
Nickel.
Cobalt.
Hydrogen.
Manganese.
Titanium.

Thus rejecting zinc and barium from Kirchhoff's list of accepted elements, adding cobalt from the doubtful list, and hydrogen and manganese from Ångström's, and titanium from his own observations.

The table of coincidences referred to, and Ångstrom's remarks thereon, explain the cause of this. Kirchhoff's evidence for zinc had depended upon the coincidence of two lines only, and these were doubtless thought insufficient, as in the cases of the metals retained in the list the number of the coincidences was much greater; viz. :—

Metals retained by Ångström.

Sodium . . .	9 (all)	Magnesium . . .	4 (3 ?)
Iron . . .	450	Chromium . . .	18
Calcium . . .	75	Nickel	33
Cobalt . . .	19	Hydrogen . . .	4 (all)
Manganese . .	57	Titanium. . .	118

Barium[1] . . .	11 (of 26)	Zinc	2 ? (of 27)
Aluminium . .	2 ? (of 14)		

From Ångström's remarks, which I proceed to give, it is evident that he was not quite satisfied with the brilliancy test relied on by Kirchhoff, and that his doubts concerning zinc arose from this cause.

Ångström's remarks on the brilliancy test.

" L'aluminium possède certainement des raies brillantes en plusieurs endroits du spectre, mais les raies situées entre les deux H sont les seuls qui semblent coïncider avec les lignes Fraunhofériennes. Pour expliquer ce phénomène singulier, il faut dire que les raies violettes se présentent comme les plus fortes dans le spectre de ce métal. De même que les raies jaunes du sodium, ces deux raies d'aluminium ont fait voir quelquefois le phénomène d'ab-

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[1] I include this "below the line," though I cannot but think that its omission by Thalén was accidental.

FOURTH PAPER.

sorption consistant en ce qu'une raie noire se présente c le milieu de chacune d'elles, ce qui prouve la forte inter des dites raies. En observant les rayons extra-violette ce métal, on décidera si les deux raies mentionnées ci sus coïncident ou non avec des raies Fraunhofériennes ; si ma supposition est vraie, les raies extra-violettes doi coïncider aussi avec les lignes obscures du spectre solai

"A deux raies du zinc que j'ai indiquées sur mes plan comme coïncidant avec des raies Fraunhofériennes il en ajouter une troisième, située à 4809·7 ; mais, à l'égard des d raies, très-larges et très-fortes, d'une apparence nébule il n'y a pas de correspondance visible ; ainsi, la prés du zinc dans le soleil me semble très-douteuse. Je c cependant qu'il existe trois raies de magnesium, du m aspect nébuleuse, que ne possèdent pas non plus correspondance avec les raies de Fraunhofer, quoiqu présence de ce corps dans le soleil ne permettre pa moindre doute."[1]

The lines in the solar spectrum are the longest.

In the accompanying maps the lines of certain met vapours reversed in the solar spectrum are given under spectrum mapped by the new method. *It will be seen invariably the reversed lines are simply those which longest in the spectrum.*

It is not necessary on the present occasion to dwell u the great importance of this determination, both in c nection with the fact just stated and the other facts tou ing the lines which remain longest in chemical comb tions[2] and mechanical mixtures. It supplies us at c with the true test to apply to the reversal of solar lines a guide of the highest value in spectrum observation the chromosphere and photosphere. It is one, doubt

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[1] It will be seen from my maps that this statement is not accu Thalén's later work left only one line doubtful.

[2] A. Mitscherlich has noted the disappearance of certain lin consequence of the presence of several substances in the same fl but he only applies this to the sun by supposing the substances t combined, and so not to give a spectrum.—*Ann. de Chim. et de* 3 sér. vol. lxix. p. 176.

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which will shortly enable us to determine the presence of new materials in the solar atmosphere, and it is seen at once that to the last published table of solar elements—that of Thalén—must be added, zinc, aluminium, and possibly strontium[1] as a result of the application of the new test.

1474K is not due to iron.

In the case of the chromosphere, the observations of the order of lengths of the bright lines is invested with a new importance, as also the observation of lines which are not reversed in the ordinary solar spectrum. As an instance of this, I may state, that the fact that the re-reversal into brightness in the chromosphere of the line 1474K is not due to iron vapour, is settled by the other fact, which this new method has enabled me to determine, that the coincident line in the iron spectrum is one of the shortest lines in the whole spectrum.

Cyclical changes in the photosphere.

In the case of the photosphere, not only may we hope to account for such cyclical changes as I have long had reason to suspect, and have referred to in prior communications to the Society, but it is essential that spot-spectra shall be photographed with special reference to the consideration that in such spectra the new lines may now be found, in all probability, to be those which are only slightly shorter than those ordinarily reversed. This research I am making arrangements to carry on.

It will be observed that in the maps the elements are arranged in the order of their atomic weights. This was done before all the comparisons were made, because, as I have before announced to the Royal Society in the case of several of the elements, the length of the lines in the spectra of the vapours observed in the chromosphere are also most frequently arranged in this order, as predicted by Mr. Stoney.[2] The comparison rendered possible by the maps also bears out this view with regard to the outer layers; for in the case of H and Na all the lines are reversed; in the case of Mg, about which there was a doubt in Ångström's observations, only one line is possibly dropped, and this is not certain. When we come, however, to the elements with higher atomic weights, the number of lines reversed is less. But the maps also show that when once the higher layers of the chromosphere, where less constant action goes on, are passed, atomic weight ceases to be a guide; and we are therefore driven to other considerations, which promise to largely increase our knowledge of the kind of action at work in the solar atmosphere and the cyclical variation of that action.

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[1] Barium also, if a *lapsus calami* has not been made.

[2] This arrangement has since been broken up for the convenience of the engraver. Some of the spectra having both sun and chloride lines had to be displaced by others without these in order to get the whole of the maps on to the three Plates

Difference between higher and lower strata of chromosphere.

The maps which accompany this communication have been made by my assistant, Mr. R. J. Friswell. They have only been revised by myself. I am anxious to take this opportunity of testifying to the zeal and ability he has displayed in a research necessarily very tedious from its character, and requiring great patience and care.

[It has been found impossible to give here all the maps and tables which appear with this communication in the *Phil. Trans.* The accompanying Plate, however, in which some of the maps are reduced, will give an idea of the results of this research.]

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Researches in Spectrum Analysis in connection with the Spectrum of the Sun, No. II.—By J. NORMAN LOCKYER, F.R.S. Received March 14, 1873.[1]

[ABSTRACT.[2]]

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THE observations in this paper are a continuation of those referred to in the previous communication bearing the same title. They deal (1) with the spectrum of chemical compounds, and (2) with the spectra of mechanical mixtures.

I. *Chemical Compounds.*

Several series of salts were observed; these series may be divided into two:—1st, those in which the atomic weights varied in each series; 2nd, those in which the associated elements varied in each series. The following salts were mapped:—

Salts whose spectra were observed.

PbF_2, $PbCl_2$, $PbBr_2$, PbI_2; SrF_2, $SrCl_2$, $SrBr_2$, SrI_2; BaF_2, $BaCl_2$, $BaBr_2$, BaI_2; MgF_2, $MgCl_2$, $MgBr_2$, MgI_2; NaF, $NaCl$, $NaBr$, NaI.

The conditions of the experiments are described; the same aluminium cups, described in the first paper, were used, and the poles were arranged in such a manner that they could at will be surrounded with any gas or vapour. Hydrogen was used in some of these experiments; it was purified in the usual manner by drying and freeing from traces of sulphuretted hydrogen; it was then passed over clean cut pieces of sodium, and admitted to the poles. An induction-spark from five one-pint Grove cells was used, the *circuit being without the Leyden jar.*

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[1] *Proc. R. S.* vol. xxi. p. 285, No. 144.

[2] This paper will appear *in extenso* in the forthcoming volume of the *Philosophical Transactions.*

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The lead compounds behaved (in air) as follows:—

The fluoride gave the eleven longest lines of the metal, but four were very faint.

The chloride gave nine lines; one of these is very short.

The bromide gave six lines, but one is a mere dot on the pole.

The iodide gave four lines distinctly, and two as dots, one of which is scarcely visible.

The lines die out in the order of their length.

It is pointed out that the decrease in length and number of lines follows the increase in the atomic weight of the non-metallic element, the lines dying out in the order of their length.

Barium was next experimented on, the same series of salts being used. A marked departure from the results obtained in the case of the lead compounds was observed, especially in the case of the fluoride, its spectrum being much the simplest; in fact, it consisted of only four lines. Strontium behaved like barium, and so did magnesium fluoride. This anomalous behaviour was found to be most probably due to the exceedingly refractory nature of these fluorides, all of them being quite infusible, and non-volatile in any spark that was used.

Anomalous behaviour of fluorides.

Sodium salts.

Sodic fluoride, sodic chloride, sodic bromide, and sodic iodide exhibited a behaviour exactly the reverse of that of lead, *i.e.* the iodide showed most of the metallic spectrum.

Flame spectra as compared with low tension electric spectra.

The difference between flame-spectra and those produced by a weak electric discharge are then discussed. Beads of the chlorides, &c., were heated in a Bunsen-gas flame; BaI_2 gave a "structure" spectrum (since proved to be due to the oxide) and the line at wave-length 5534·5, by very far the longest metallic line of barium. The bromide behaved like the iodide, and so did the chloride, except that its spectrum was more brilliant. Baric fluoride gave scarcely a trace of a spectrum, the oxide structure being scarcely visible, and 5534·5 very faint indeed. The strontium salts follow those of barium—4607·5, the longest strontium line appearing in conjunction with an oxide spectrum. The strontic fluoride, however, refused to give any spectrum whatever. These results are compared with those obtained with the weak spark, and it is shown that the difference is one of degree; *e.g.* baric bromide gives 25 lines in the spark; these are the longest lines. In the flame it gives but one line; but this is the longest of all the barium lines, and indeed very far exceeds all the others in length. When the flame-spectra are compared with those produced by the low tension spark, the spectra of the metals in the combination are in the former case invariably more simple than in the latter, *so that only the very longest line or lines are left.*

Spectra of oxides seen in all flames.

Some experiments made by Mr. R. J. Friswell to determine the cause of the similarity of the spectra of the various salts of the same metal observed in air are then given, the conclusion being that the spectrum observed is really that of the oxide.

Results obtained by other observers.

Kirchhoff and Bunsen's, Mitscherlich's, and Clifton and Roscoe's prior conclusions on the points investigated are stated at length; and it is shown that the observations recorded, taken in conjunction with the determination of the long and short lines of metallic vapours, are in favour of the views advanced by Mitscherlich, Clifton and Roscoe. For while the spectra of the iodides, bromides, &c., of any element in air are the same as stated by Kirchhoff and Bunsen, the fact that this is *not* the spectrum of the metal is established by the other fact, *that only the very longest lines of the metal are present, increased dissociation bringing in the other metallic lines in order of their length.*

Spectra in an atmosphere of hydrogen.

The spectra have been mapped with the salts in hydrogen: here the spectra are different, as stated by Mitscherlich; and *the metallic lines are represented according to the volatility of the compound, only the very longest lines being visible in the case of the least-volatile one.*

The following are the conclusions arrived at:—

1. A compound body has as definite a spectrum as a

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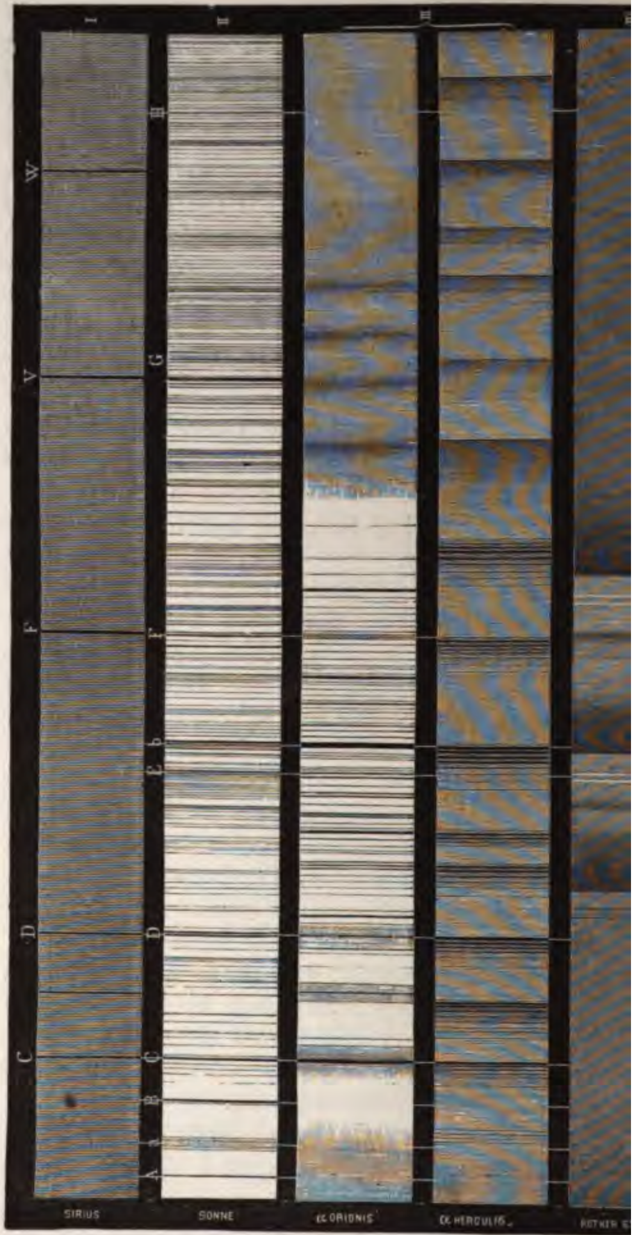
I
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α ORIONIS
α HERCULIS.
ROTHER S

simple one; but while the spectrum of the latter consists of lines, the number and thickness of some of which increase with molecular approach, the spectrum of a compound consists in the main of channelled spaces and bands which increase in like manner. In short, the molecules of a simple body and of a compound one are affected in the same manner by their approach or recess, in so far as their spectra are concerned; *in other words, both spectra have their long and short lines or bands.* In each case the greatest simplicity of the spectrum depends upon the greatest separation of molecules, and the greatest complexity (a continuous spectrum) upon their nearest approach.

Dissociation of compound molecules.

2. The heat required to act upon a compound, so as to render its spectrum visible, dissociates the compound according to its volatility; the number of true metallic lines which thus appear is a measure of the dissociation, and doubtless as the metal lines increase in number the compound bands thin out.

Mitscherlich's observations, that the metalloids show the same structural spectra as the compound bodies, is then referred to, and the question is asked whether the molecules of a metalloid do not in structure lie between those of [metallic] elements on the one hand and compounds on the other.

These considerations are applied to solar and stellar spectra; the general appearance of the solar spectrum shows that in all probability there are no compounds in the sun.

Secchi's maps of a large number of stellar spectra are referred to as now indicating beyond all doubt the existence of compound vapours in the atmospheres of some stars; and it is suggested that the phenomena of variable stars may be due to a delicate state of equilibrium in the temperature of a star which now produces the great absorption of the compound and now that of the elemental molecules.

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II. *Mechanical Mixtures.*

The second part of the paper deals with mech
mixtures. Maps of the spectra of alloys of the foll
percentages are given:—

Sn and Cd percentages of Cd 10·0, 5·0, 1·0, 0·1
Pb and Zn ,, Zn 10·0, 5·0, 1·0, 0·1
Pb and Mg ,, Mg 10·0, 1·0, 0·1, 0·0

Quantitative spectrum analysis suggested.

It is pointed out that the lines disappear fro
spectrum as the percentage becomes less, the sh
lines disappearing first; and that, although we hav
the foreshadowing of a quantitative spectrum analys
method is so rough as to be inapplicable. It is
stated that further researches on a method which pr
much greater accuracy are in progress.

The bearing of these results on our knowledge
reversing layer of the sun's atmosphere is then disc

III.—NOTES.

NOTE A.[1]

SPOT PHENOMENA AND THEORIES.

NOTE A.

The following pages contain a *résumé* of the observations and theories of various investigators of the spectrum of solar spots. My own observations and theories it will be unnecessary to refer to again, as they have already been given.

My first observation was made in 1866 (see p. 435). In 1867, Mr. Huggins stated to the Royal Astronomical Society that in the spots no certain modification of the solar spectrum had been detected.[2] In 1868, however,[3] in a paper read before the Royal Society on May 14th, after stating that he had examined the whole spectrum from A to G and found no lines in the spots which were not present in the ordinary spectrum of the sun, he observed that the absorption was both continuous and selective, as I had already done, the lines C and F being but slightly intensified, while the group of chromium lines a little less refrangible than *b* was especially marked by increased strength. The same was the case with D, of which Mr. Huggins remarked: "These lines appeared slightly broader, as if by the addition of a faint and narrow nebulosity at both sides."

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[1] See p. 438.
[2] *Monthly Notices*, *R.A.S.*, vol. xxvii. p. 131.
[3] *Phil. Trans.*, 1868, p. 553 *et seq.*

NOTE A.

He further mentioned the lines B, *b*, E, many
iron lines in Kirchhoff's map, and probably 206
2067·1 (Kirchhoff), as participating in the increased s

Father Secchi, whose first idea seems to have b
the spots contained aqueous vapour—about whic
presently—communicated, in 1869, several paper
Paris Academy of Sciences, in which he makes
lowing remarks on the phenomena observed :—

> "Lorsqu'on place une tache dans le champ du spectrosco
> sceau des raies correspondantes se présente à peu près com
> 1° les raies noires qui sont très-fines et très-nettes dans notre i
> sur le reste du Soleil, paraissent se gonfler et s'élargir à
> tache; leur bords ne sont plus tranchés nettement, comm
> reste du spectre; 2° un grand nombre des raies très-fines
> visible ailleurs deviennent très-larges et nébuleuses, comme j
> indiqué dans une autre communication; 3° toute l'harmonie
> sité relative des lignes brillantes se trouve profondémen
> et, pendant que quelques-unes diminuent énormément d
> d'autres traversent toute la tache et même le noyau, sans s
> 4° dans celles que paraissent s'affaiblir, cet effet est dû plu
> empiétement des lignes noires élargies qu'à une diminution
> lumière. Ainsi dans la dernière tache, l'élargissement
> D′ et D″ était si grand, que l'interval lumineux disparaissai
> complètement, pendant que, avec notre fort instrument, elle
> très-séparées et très-nettes en dehors de la tache."[1]
>
> "Mais une classe de phénomènes encore plus intéressante
> mon attention : la zone obscure qui se développe par absorp
> les noyaux entre les raies D et C m'a fait chercher s'il n'y en
> d'autres. Effectivement, j'ai constaté que, dans quatre re
> spectre, cette absorption devenait plus sensible que dans le
> l'une de ces régions se trouve dans le rouge, près de C du c
> 2° une autre près de la raie D; 3° un espace assez vaste dan
> et ce qui est plus remarquable, j'ai observé que sur le fond
> nébulosité sombre brillaient des raies lumineuses, séparée
> deux par des intervalles médiocres, qui échappaient évide
> toute absorption; 4° enfin une autre bande dans le bleu, près
>
> "J'ai dit d'abord que les raies 7 et 10 de Van der Willige
> très-modifiées; mais ce ne sont pas les seules : il y en a
> nombre d'autres qui le sont de la même manière. Ces d
> appartiennent au calcium. Des phénomènes pareils se dé
> dans le groupe voisin du fer, et surtout dans le groupe com
> les raies 1207 et 1241 de Kirchhoff et dans celui dont le m
> respond à la raie 1421 de Kirchhoff. Ces raies deviennent p
> et restent bien tranchées. Or beaucoup de ces raies appa
> au fer, et j'en ai identifié un grand nombre. Au contraire

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[1] *Comptes Rendus*, vol. lxviii. p. 764, 20th March, 186

[2] Op. cit., vol. cit., p. 961, 26th April.

> du magnésium ne sont que très-faiblement influencées ; les raies du sodium qui s'élargissent, mais deviennent nébuleuses aux bords et en quelques autres points, sont peu influencées. De là, on pourrait conclure que ces vapeurs sont, à des hauteurs différentes, en proportion très-diverses."[1]

He also states that the aspect of the spectrum of spots, especially on the penumbræ, is like that of certain stars, and then *that the spectrum of a spot resembles that of the limb* :—

> "Ayant examiné comparativement le spectre du noyau des taches et celui du bord du disque, du côté intérieur, je suis arrivé à la conclusion que ces deux spectres se ressemblent considérablement. L'élargissement des raies constaté dans les noyaux se reproduit près du bord, de sorte que, dans cette région, il égale souvent celui qu'on voit dans les taches les plus légères et les plus superficielles."[2]
>
> "Ainsi se trouve confirmée indirectement cette assertion, que l'absorption qu'on remarque dans les noyaux des taches n'est pas due à des masses étrangères qui flotteraient au-dessus de la photosphère, mais seulement à une plus grande profondeur de l'atmosphère traversée, car le même effet se produit près du bord par la simple intervention d'une plus grande épaisseur de cette atmosphère même."[3]

This however he has now (1873) abandoned.

The next statement is that there are no new lines in the spots :—

> "Nous avons vu que, dans ce spectre, il n'y a pas production de raies fondamentales nouvelles, mais seulement un renforcement considérable des raies solaires connues déjà existantes."[4]

In his description of the spectrum of a solar spot, April 9th, 1870, Professor Young states :—[5]

> . . . . "Many of the dark lines were widened and deepened in this nucleus spectrum in the manner which the description and figures of Mr. Lockyer have made familiar. Many also were unaffected. Among these were notably *a*, B, E, 1474, the four lines of *b*, 1691 and G.
>
> "The two sodium lines D_1 and D_2, and 850 (Fe) were distinctly, but not greatly, widened.
>
> "The effect was most marked upon the following :—864 (Ca), 877 (Fe ?), 885 (Ca), 895 (Ca and Li), 1580 (Ti), 1599 (Ti), 1627 (Ca), and 1629 (Ti). I have marked 877 doubtful, because there lies very near it a line whose origin is unknown, and I am not sure to which of the two the thickening was due. The Titanium lines are identified as

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[1] *Comptes Rendus*, vol. lxviii. p. 962, 26th April, 1869.

[2] Op. cit., vol. lxix. p. 40, 5th July.

[3] Op. cit., vol. cit., p. 41, 5th July.

[4] Op. cit., vol. cit., p. 166, 19th July.

[5] *Journal of Franklin Institute*, No. 535, p. 64, July 1870.

NOTE A.

such by reference to Ångström's Atlas. I was greatly surprised prominence they assume in the spot-spectrum, as they are spicuous in the normal spectrum ; and a similar remark applies calcium lines.

"I do not intend to convey the idea that the lines mentione the only ones that were much deepened ; there were many mostly faint, affected to nearly the same degree, but I had not t identify them."

But it is to the brilliant results of his expediti Mount Sherman that I am most anxious to call atte He states:—[1]

"The spectra of several different spots were carefully studied, catalogue was drawn up of 155 lines which are more or less af usually by being greatly widened, but in some cases by a wea or reversal. Several bright lines were also found in the spot-spe

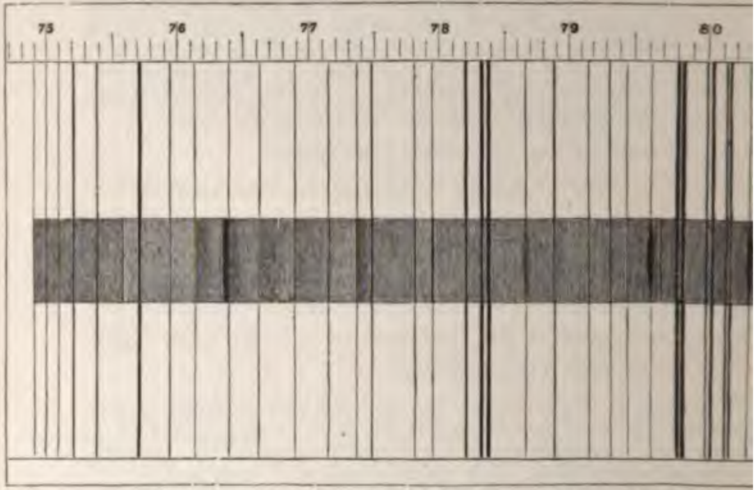

FIG. 156.—Young's observation of the spectrum of a solar spot between C and

and between C and D some very peculiar shadings terminated s at the less refrangible limit by a hard dark line, but fading out ally in the other direction at a distance of three or four of Kirc scale divisions. The interpretation of such markings is not quit but would rather seem to point to such a reduction of temp over the spot-nucleus as permits the formation of gaseous com by elements elsewhere dissociated, since the shaded spectra ar probably characteristic of non-elementary substances, a view f by Schuster's recent beautiful investigations upon the spect nitrogen."

---

[1] *Nature*, December 12, 1872.

With regard to the reversal of the Calcium lines H¹ and H², he adds :—

> "It was found that these two lines (not the hydrogen lines, as has been erroneously reported) are also usually, and I am pretty confident always, reversed in the spectrum of sun-spots, not so clearly, moreover, in the nucleus as in the penumbra, and over a somewhat extensive region surrounding it. This reversal of the H lines does not involve at all the disappearance of the dark shade, but a bright streak rather than a line makes its appearance in the centre of the shade, which itself is, if anything, a little intensified."

Prof. H. C. Vogel[1] gives the following list of lines seen in the spots :—

660·4	Fe		599·0	
653·1			594·8	Fe
651·5		very much thickened.	589·5	Na (D)
647·6			588·9	
646·2	Ca		587·4	
645·4			585·6	Ca
644·9	Ca		578·9	Fe
643·0	Fe		579·0	Fe
641·1	Fe		578·5	
640·7	Fe		576·2	Fe
634·6			575·9	
641·4	Fe		575·4	Fe
629·4			573·0	Fe
629·3			572·6	
629·1			571·7	
629·0			570·8	Fe
628·2			570·5	
628·0			570·1	
627·6			568·7	Na
624·0			568·1	
623·9			566·4	
623·7			566·1	Fe
623·6			565·8	Fe
619·1			518·3	Mg (*b*)
617·2	Fe		517·2	
616·8	Ca		516·7	
616·2	Ca	very much thickened.	495·7	Fe
616·1	Ca		492·0	Fe
614·4			491·8	Fe
613·6	Fe	much thickened.		
612·1	Ca			
610·1	Ca			
606·4	Fe			
600·7	Fe			

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[1] H. C. Vogel, "Beobachtungen angestellt auf der Sternwarte des Kammerherrn von Bülow zu Bothkamp." Heft 1. 1872.

Theories.

NOTE A. After Mr. Huggins had convinced himself th
really was a difference between the ordinary sola
spot spectrum, he states :—

> “It may be well to consider some of the conditions of
> surface by which the phenomena observed may have been
> A cooler state of the heated vapours by which the lines of
> are produced, would diminish the radiation from the gas itse
> leave more completely uncompensated the absorption by t
> the light from behind it. This cause would produce increas
> ness of the lines, but would not account for more than a sli
> rent increase of breadth. The greater breadth of the lines
> point rather to a condition of the gases in which their power
> tion embraces for each line an increased range of wave-leng
> the power of absorption of gases varies in this respect is
> the increase of breadth which some of the bright lines of s
> assume under altered conditions of tension and temperatur
> be sufficient to refer to the expansion of the lines of hydrog
> tension increases. A similar increase in the range of its
> absorption on light passing through it should take place und
> conditions of density and temperature.
>
> “The phenomena may point to an increase of density in th
> existing within the umbra. Such a state of things would n
> exist at a point somewhat nearer the sun's centre ; but w
> know through how great a depth of gas below the photos
> receive the light which comes from the umbra. Our view
> point will be connected with the interpretation we give to M
> discovery of the existence within the umbræ of spots of a s
> part almost wholly devoid of light. Does this nucleus represe
> complete unveiling of the inner part of the sun ? or does it s
> cooler and less luminous part of the down-rushing solar atr
> The latter suggestion, which is in accordance with the expl
> sun spots proposed by Dr. Balfour Stewart, would seem to
> lower temperature with the broader lines of absorption.”[1]

Dr. Frankland and myself have, I think, nov
beyond all question that the phenomenon is reall
the main to an increase of density.

Let us now consider the explanations and
adopted by Secchi as regards the phenomena
One of the first theories he takes up is that th
ences in the spectrum which he saw was due to
that the vapour of water—steam—exists in the sp

---

[1] *Phil. Trans.*, 1868, pp. 552—554.

"Enfin je crois même avoir vu des traces de vapeur d'eau dans le soleil, et surtout dans le voisinage des taches. On voit là les mêmes séries de raies nébuleuses que lorsque les cirrus vont traverser le champ de la lunette. Mais cela demanderait à être appuyé par des nouvelles observations."[1]

"Dans la dernière communication . . . . je vous annoncais que je croyais avoir constaté la présence de la vapeur d'eau dans le voisinage des taches solaires, en ajoutant toute fois que cela demanderait des recherches nouvelles. Ayant eu dernièrement de belles journées, j'en ai profité pour analyser le phénomène, et je vais l'exposer en détail vu sa grand importance."

* * * * * *

"D'après ces résultats, il paraît clair que la vapeur d'eau existe dans l'atmosphère solaire, au voisinage des grandes taches : il reste seulement à vérifier la constance de ces phénomènes et si elle se vérifie pour tous, car j'ai rencontré des taches très-petites et très-noires qui ne la présentaient pas."[2]

Alluding to certain nebulous lines in Kirchhoff's map, he says:—

"On ne connait pas les substances que les produisent, mais quelques-uns d'entre eux sont dus sans doute à la vapeur d'eau, surtout dans l'orangé et la jaune."[3]

At the same time that he was promulgating these views as to the presence of water in spots, he was defending the selective absorptive nature of the phenomena from the attacks of Professor Respighi, who considered it might be accounted for instrumentally :—

"Le phénomène que nous venons de signaler n'est donc pas l'effet d'une simple diminution de lumière dans le fond, mais il est bien dû à une faculté absorbante élective plus intense, qui réside à l'intérieur des taches. En effet, nous savons que les taches sont des cavités dans le photosphère : dans leur intérieur, la couche absorbante doit être plus épaisse, et altérer beaucoup de rayons que ne sont pas absorbés par l atmosphère extérieure. Les lignes brillantes qui traversent souvent les noyaux pourraient bien être les lignes directes de ces gaz que j'ai signalés comme constituant la masse gazeuse intérieure du soleil dès le mois de janvier 1864 (voir 'Bull. Météor. de l'Observatoire,' 31 janv. 1864, vol. iii. p. 4).

"Cet effet serait alors complètement semblable à celui qu'on observe dans l'atmosphère terrestre, où une couche plus épaisse près de l'horizon produit l'élargissement de lignes fines, et donne à un grand nombre d'autres lignes, à peine visible quand le soleil est assez haut, un

---

[1] *Comptes Rendus*, vol. lxviii. p. 238, 1st Feb., 1869.
[2] Op. cit., vol. cit., pp. 358—360, 15th Feb.
[3] Op. cit., vol. cit., p. 1084, 10th May.

NOTE A. aspect nébuleux. Telle me paraît l'explication plus rati
phénomène qu'on observe en analysant les taches solaires
troscope."[1]

It is clear, then, that he does not accept the
density theory.

In the following Secchi suggested that the spot
may give a first order spectrum :—

> "Or, il serait impossible, dans le Soleil, de distinguer
> espèce de lumière que celle des raies brillantes des spectres
> ième ordre : celles du premier ordre sont trop faibles e
> tout au plus produire les zones plus obscures que nous a
> statées dans les noyaux des taches."[2]

With regard to later discussions which have bee
on while this book has been going through the
may just say that Secchi in his book on the sun,
in several discussions with M. Faye, had insisted
spots being centres of eruption, and considered
expansion of the erupted masses of gas caused a co
such an extent that absorption was produced by th
gases resulting from this cooling. After, however
sing with M. Faye that author's new theory of
action as the prime producer of spots, he has cha
ideas, and considers the spots as being themse
erupted masses floating on the surface of the su
following quotation, which I give in the original
will show how complete and how sudden was this
of front :—

> "Io in quest' opera [his book on the Sun,] mettava il ce
> eruzioni nel centro delle macchie ; questo non è dimonstrato
> ha bisogno, di prove più positive. Lo studio recente mi fa cr
> la macchia è formata principalmente dalla massa dei gaz e
> provvenienti dalla eruzioni, questa massa informa da princip
> poco a poco regolarizzando e prendendo una forma circol
> tanto che in progresso di tempo essa viene divisa dalle corr
> mostra l'osservazione."[3]

The following statement by Secchi, in reply
also bears upon the same point :—

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[1] *Comptes Rendus*, vol. lxviii. p. 765, 29th March, 1869.

[2] Op. cit., vol. lxx. p. 84, Jan. 10th, 1870.

[3] *Memorie della Societa degli Spettroscopisti italiani*, vo
p. 116.

NOTE A.

"M. Faye continue : 'Les aches ne sont plus que le réceptacle des produits trop lourds pour être entraînés par l'hydrogène au-dessus de la chromosphère.'

"Ce n'est pas là l'expression exacte de ma pensée : les taches sont bien, selon moi, dues à des amas de vapeurs émises par éruption et refroidies ; mais je n'ai pas dit que l'hydrogène ne pouvait les entraîner, ni qu'elles ne pouvaient dépasser la chromosphère. Comment aurais-je pu le penser ayant vu le magnésium et le sodium élevés jusqu'à une et quelquefois deux minutes. Habituellement même, dans les masses vives, on les voit dépasser considérablement la chromosphère. Je n'ai pas davantage considéré l'hydrogène comme étant le véhicule des autres vapeurs : j'ai dit seulement que les vapeurs métalliques sortent mêlées à l'hydrogène, mais je crois qu'elles pourraient également sortir par leur propre force, et sans l'aide de ce gaz."[1]

M. Rayet considers that though the spots may owe their formation to an ascending current, according to M. Faye's first theory, yet that a descending current is the more probable. That if the hot atmosphere surrounding the cloudy photosphere rushes down upon it, the clouds will dissolve, and the brighter portions will then appear as a facula ; and then if the hot current penetrates still deeper, the photospheric rain will cease, and be replaced with a bed of clouds, thus causing a penumbra, the centre of which being still hotter, stopping all condensation, will produce a spot nucleus. He then adds :—

"Cette dernière hypothèse en faisant dépendre la formation des taches de l'action d'une couche extérieure à la photosphère, montre parfaitement que les facules doivent précéder les taches et se trouver accumulées autour d'elles : ce dont l'hypothèse de la cause interne, ne rend que difficilement compte."

He adds that we have direct proof of these descending currents, and refers to Secchi's observation of the prominences inclining towards a spot, and to my own of the rush of the willow leaves into a nucleus. He then adds :—

"Si en effet le noyau des taches était formé d'un nuage de particules solides froides, ce noyau serait absolument obscur ; or, l'analyse spectrale de cette région m'a montré que le noyau possédait une lumière propre très-sensible et que, s'il était moins brillant que la masse de la photosphère, cela provenait d'un accroissement dans l'obscurité des lignes noires du spectre solaire ordinaire, accroissement identique à celui qui résulterait d'une augmentation dans la densité des

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[1] *Comptes Rendus*, vol. lxxvi. p. 912, April 14th, 1873.

NOTE A.

vapeurs métalliques qui renferme l'atmosphère solaire ; or, c'e
cisément un accroissement de cet ordre que doit produire la diss
des nuages photosphériques par un courant chaud."[1]

NOTE B.[2]

MR. HUGGINS AND THE NEW METHOD.

NOTE B.

Soon after I began to observe the sun telescopical
1861, with my 6¼ object glass (a *chef-d'œuvre* ma
Messrs. Cooke, of York) which I received in that
I had the pleasure of making the acquaintance o
(now Sir William) Grove, who informed me that for
years he had been attempting to observe the solar
minences by means of red glass in ordinary sun
stopping out the sun's image in the telescope by a su
diaphragm. When, therefore, I published my pap
1866, in which was contained the idea of rendering
visible by means of a spectroscope, I looked upon the
glass method (which I had myself employed) as so
known and obvious that I did not consider any refe
to it necessary.[3]

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[1] M. G. Rayet, "Memoire sur les raies brillantes du Spectre d
mosphère solaire, et sur la constitution physique du Soleil." Sept
1871, pp. 76-78.

[2] See pp. 444, and 446.

[3] I have elsewhere stated that this idea of thus using the sp
scope occurred to me in a conversation with Dr. Stewart. A
Huggins has stated, in the Council of the Astronomical Society
there is nothing in my paper to show that I had any conception
method I afterwards successfully employed, I append an extrac
communication by Dr. Stewart on this subject :—

"Permit me to state that to Mr. Lockyer undoubtedly belon
credit of having known this method, even before his preliminary
in 1866, in which it was sufficiently published. We had num
conversations on the subject, and when he showed me the MS.
preliminary paper, and told me at the same time that he was ab
ask a grant for the purpose of procuring a more powerful spectros
I advised him to introduce his views in the shape of a question,
he accordingly did in the following terms :—'May not the spectro
afford us evidence of the existence of the "red flames," which
eclipses have revealed to us in the sun's atmosphere, although

In February, 1868—nearly a year and a half after my paper—Mr. Huggins presented an account of the work done in his observatory to the Anniversary Meeting of the Royal Astronomical Society. After stating that he had succeeded in constructing a new spectroscope with dispersion equal to about seven prisms of dense flint-glass of 60°, which, be it remarked, is more powerful than the spectroscope I have usually employed in my researches up to the present time (1873), the report proceeds:—

> "Mr. Huggins intends to make use of this instrument in the continuation of his observations on the spectra of different parts of the sun's surface, and of the solar spots. He has already insulated the spectrum of the umbra of a spot. During the last two years numerous observations have been made for the purpose of obtaining views of the red prominences seen during a solar eclipse. The invisibility of these objects at ordinary times is supposed to arise from the illumination of our atmosphere. If these bodies are gaseous, their spectra would consist of bright lines. With a powerful spectroscope, the light reflected from the sun's edge would be greatly reduced in intensity by the dispersion of the prisms, while the bright lines of the prominences, if such be present, would remain but little diminished in brilliancy. This principle has been carried out by various forms of prismatic apparatus, and also by other contrivances, but hitherto without success."

In October 1868, I was fortunate enough, as described in p. 446, to obtain a spectroscope which, although of less dispersive power than the one then in use by Mr. Huggins, enabled me, on the first day of its use, to observe the lines of a solar prominence.

---

escape all other methods of observation at other times? And if so, may we not learn something from this of the recent outburst of the star in Corona?'

"I gave this advice to my friend, Mr. Lockyer, because I thought that, as it might be some time before he obtained the new instrument, it might be well that he should publish what I conceive would enable him to claim for himself the knowledge of this principle. And I think that anyone well acquainted with spectra, on reading the question put, could not fail to see what was meant; and, if he were previously ignorant of the principle, he could not fail to perceive it. I therefore feel rather astonished that anyone should claim the statement made by Mr. Huggins two years afterwards as being the commencement of a new principle."—*Letter of Dr. Balfour Stewart*, in *Nature*, vol. vii. p. 301, February 20th, 1873.

[1] *Monthly Notices, R.A.S.*, vol. xxviii. p. 88, Feburary 14th, 1868.

NOTE B. Mr. Huggins at once addressed a communication to the Astronomical Society, in which the following passage occurred:—

> "The observations of the eclipse of August last having shown the position of the bright lines of the red flames, Mr. Lockyer and M. Janssen succeeded independently in viewing the spectra of these objects."[1]

The paper goes on to describe Mr. Huggins' work with absorptive glasses of a similar tint to that to which I have referred in the first paragraph of this 'Note.'

To this attempt to take away all credit from M. Janssen and myself, I felt bound to send a reply, as the insinuation was as unfounded as it was unworthy. Here it is:

> "In the paper referred to I read as follows:—'The observation of the eclipse of August last, having shown the position in the spectrum of the bright lines of the red flames, Mr. Lockyer and M. Janssen succeeded independently by a similar method in viewing the spectra of these objects.'
>
> "The obvious meaning of the paragraph is that my work was based upon, and that my success was due to, the observations made during the eclipse in India.
>
> "I therefore beg permission to show—I. That my work was not based upon the Indian observations; and II. that my first success was in no way due to them.
>
> "I. In the first place, I may remark that my work was begun in 1866, and the method I adopted was clearly indicated in a paper communicated to the Royal Society in that year. My observations were then unsuccessful, owing to the small dispersive power of the instrument employed, which I have since ascertained was utterly incapable of showing the bright lines.
>
> "Secondly, I attempted to remedy this state of things by using an instrument of greater dispersive power, and I obtained a grant for the construction of such an instrument, which was ordered in January, 1867.
>
> "Thirdly, after a long series of unfortunate delays, due to instrument makers, and to my own severe illness, the instrument was received in an incomplete state on the 16th of October last (Friday night.)
>
> "Fourthly, the discovery of the prominence spectrum was made on the 20th of October (Tuesday morning), *i.e.* four days after the instrument was received, and on the very first day I gave to the observation after the instrument had been mounted, adjusted, and tested.
>
> "Fifthly, my method of observation was to clamp my telescope on a particular part of the sun's limb, and then to sweep along the spectrum very slowly (three adjustments being necessary), from red to violet,

---

[1] *Monthly Notices, R.A.S.*, November 13th, 1868.

then to clamp on another part and work back, and then to begin again.

"II. We now come to the second point. In what I have hitherto stated I have shown that the coincidence in time between my own results and the receipt of the information from India *was due to the date on which the new instrument was received.* I am convinced that had the new spectroscope been received a year ago, the discovery would have been made just as easily; and Father Secchi's remark on the ease of the observation, even with an instrument of moderate dispersive power, strengthened this view. I have also stated my method of work.

"Now let us see how the Indian observations were the cause of my success, or if indeed they could have caused the success of anyone who depended upon them.

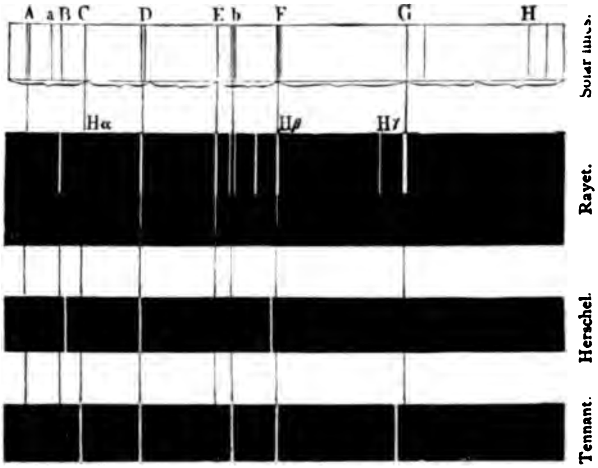

FIG. 157.—Rayet's, Herschel's, and Tennant's observations.

"On the 20th October, I knew of three sets of observations: Rayet's, Herschel's, and Tennant's.

"Rayet gave B D E *b* F
and four more undetermined lines.

"Herschel gave $\left\{\begin{matrix}\text{near}\\ \text{B or C}\end{matrix}\right\}$ D $\left\{\begin{matrix}\text{near}\\ \text{F}\end{matrix}\right\}$
declaring against F, and hesitating very much to assign even an approximate place to the line in the red.

Tennant gave C D *b* $\left\{\begin{matrix}\text{near}\\ \text{F}\end{matrix}\right.$
believed he also saw a line near G.

"The above table will, I think, show how utterly useless the Indian observations were for the purpose Mr. Huggins has assigned to them. In fact, every lettered line, except A and H, is named. Had I indeed

NOTE B.

> worked with them at all, I should naturally have looked for the line D, as that is the only line in which all the observations correspond. I distinctly referred to hydrogen (= C and F) in my paper of 1866, but in 1868 only one observer is sure of C, and only one observer is sure of F; and now it is stated that my success is due to the Indian observations.
>
> "I have sent this paper to the Royal Astronomical Society, not because any credit due to M. Janssen and myself is lessened by Mr. Huggins, paper, but because I know from experience that its *Monthly Notices* are one of the most important sources from which the history of astronomy is written. I naturally, therefore, prefer that a true account of the recent discovery should appear in the *Monthly Notices*, instead of a statement incorrect in fact, which, were it left unnoticed, would mislead those who come after us."[1]

Up to the present time—I state it with regret—Mr. Huggins has neither substantiated his injurious statement nor withdrawn it.

NOTE C.[2]

ABSORPTION AT THE LIMB.

NOTE C.

At p. 205 will be found a quotation from Ångström referring to Forbes' observation on this subject. In my paper (p. 478), although I did not state the position in the spectrum of the selective absorption which I held to be possible, I was of opinion that a selective absorption, in the case of the violet rays at least, was almost certain, from the fact that actinically the difference between limb and centre is apparently greater than it is visually. This idea I mentioned to Mr. Rutherford some time ago, but he properly reminded me of the very great influence photographic processes might have in producing the effect I had noticed.

It is nevertheless, I think, true; and Herr Vogel has recently made some detailed observations of great interest bearing out this view.[3]

In addition to this absorption in the violet, Mr. Hastings, of the Sheffield Scientific School (needless to add U.S.), has recently succeeded in obtaining evidences of the increased selective absorption in the visible part of the spectrum. Upon comparing the spectra of the centre and the limb, he remarks :—[1]

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[1] *Monthly Notices, R.A.S.*, vol. xxix. p. 4, 1869.

[2] See p. 478.

[3] See *Naturforscher, Jahrgang* V., p. 321, Oct. 1872.

"Certain differences are recognized besides that of intensity, by far the most marked of which are exhibited by the lines b_1 and b_2, which become sharper and less hazy near the limb. The line b_3 possesses the same characteristic, but to a less degree; C and F also become sharper in the same region. Excepting these and the D lines, it requires very close examination to detect any variation. There is, however, a line in the red at 768·1 of Kirchhoff's scale which is strongly marked near the centre of the sun's disc, but disappears entirely, to my power at least, within 16" to 20" from the limb. Two other lines below F, at 1828·6 and 1830·9 of the same scale, exhibit nearly complementary phenomena, *i.e.*, they are strongly marked near the edge, but much fainter at the centre. These latter lines also become greatly strengthened over the penumbræ of spots. The line 768·1 is not thus affected. These are all the differences which I have invariably seen repeated in examinions since February 17.

"Others have, however, been suspected. Certain lines, which are strengthened in a region of spots like those above mentioned, appear to be strengthened also near the edge, but do not undergo so marked a change. It is obvious that the differences should be most pronounced in the clearest sky, and such is the case. The closest examination has extended only from B to a short distance above F, as the plate glass of which the small prism is made has a decided yellow tint, and absorbs the blue rays strongly.

"Since the light from the border of the sun undergoes a general absorption, which reduces its intensity to much less than one-fourth that at the centre, according to Secchi's measurements, and yet the spectroscopic character is changed so slightly, it is impossible for me to escape the conviction that the seat of the selective absorption, which produces the Fraunhofer lines, is below the envelope which exerts the general absorption. But the phenomena of the faculæ prove not only that this envelope rests upon the photosphere, but also that it is very thin. The origin of the Fraunhofer lines, then, must be in the photosphere itself, which is in accordance with Lockyer's views.

"Any effects which the chromosphere might produce we would anticipate finding most evident in the lines of those gases which are readily detected there. A reference to the observations shows at once a compliance with this anticipation in the lines of hydrogen, magnesium, and sodium. The line 768·1 is not less strikingly in concordance, if it be regarded as 768·?* (the ? indicates doubt as to the tenths of the scale, and * absence of a corresponding black line) of Young's Catalogue of Chromosphere Lines. The lines 1828·6 and 1830·9, with others of the same class, probably have their origin in the medium which exerts the general absorption, and thus are allied to our telluric lines. It also seems probable that the chromosphere is too transparent

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[1] *Nature*, vol. viii., p. 77, May 22nd, 1873.

NOTE C. to reverse many of its lines. That this is the case in the helium lines is tolerably certain."

These results of Mr. Hastings' are of the highest value. It will be seen from pp. 475 and 478 that this part of the inquiry early occupied my attention, so far as the construction of instruments was concerned. I had, however, postponed this branch of the research, and I am glad Mr. Hastings has taken it up.

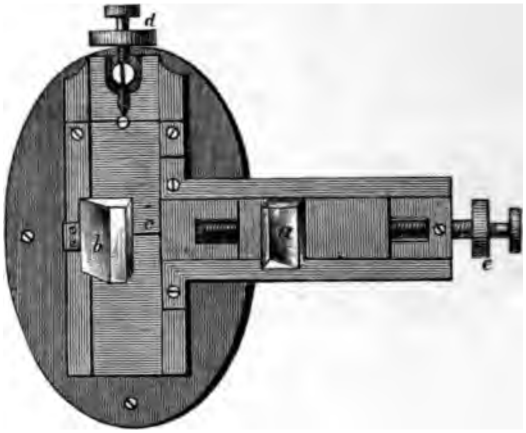

FIG. 158. *a* right-angle prism on which the image of the limb falls; *b* right-angle prism which receives a light from *a*: *c* slit through which centre of sun is observed; *d* screw to adjust width of slit; *e* screw to adjust distance of *a* from *b*, according to the semidiameter of solar image employed.

In addition to the instrument figured at p. 475, which was also early suggested by Zöllner, I have had another

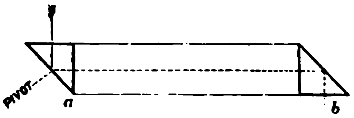

FIG. 159.—Path of the ray.

slit prepared, which I find so convenient, either for comparing centre and limb, or observing the prominences without moving the telescope either R.A. or Declination, that I give a drawing of it here.

Mr. Hastings' method of obtaining the same result is thus stated by himself:—

"I have constructed a small prism with four polished sides, its bases being parallelograms. This is so placed that one face rests upon the slit-plate of the telespectroscope, and has its acute edge perpendicular to the slit at its middle point. The instrument may then be directed so that the image of the sun falls with its centre on the uncovered portion of the slit, while the light which forms the edge of the sun, falling perpendicularly upon the first surface of the prism, suffers two interior total reflections and a displacement depending upon the form of the prism. A glance at the figure, in which s s' is the slit, L L' the diameter of the sun's image, and P the prism, shows that no light from the covered part of the slit will reach the collimating lens except that which has been reflected from the two sides of the prism. The relation of the acute angle (v) and the distance between the reflecting sides (t) to the focal length of the great telescope (F) and the width of the spectrum (a) is given by the formula,

$$2t \sin v = \text{F} \tan 16' - a.$$

The sides of the prism not fixed by the equation admit of considerable latitude, but should be made to approach the lower limit in order that the planes of the direct and transmitted images may be as little separated as possible. Of course t and v should be so proportioned that the reflections may be total.

"When the instrument is properly directed and in adjustment, we see a very narrow black line dividing the spectrum longitudinally into two parts of widely different intensity; the fainter, belonging to the limb of the sun, is marked on its edge by the bright chromosphere lines.

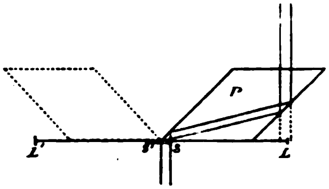

FIG. 160.—Mr. Hastings' arrangement or comparison of spectra.

"In the apparatus described, two similar prisms were also placed over the slit in a symmetrical position. The spectra of two opposite edges of the sun were thus brought together, and the change in refrangibility due to the sun's rotation was very clearly shown."[1]

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[1] *Nature*, May 22, 1873.

NOTE D.[1]

THE METHODS OF VIEWING THE FORMS OF THE PROMINENCES.

NOTE D.

The following extracts will serve to place before the reader the history of the method now almost universally in use for making observations of the forms of the solar protuberances.

In my first paper (see p. 441) I gave some drawings procured by causing the slit to pass slowly over the prominence. By this means a number of sections of varying length was obtained, which, placed side by side, gave an idea of its shape. The slit then used was extremely narrow, and was radial to the sun's limb. It was obvious that in this way the true shape of the prominence could not be seen unless the slit were moved with sufficient rapidity to allow of persistent images. At the very outset Janssen and myself attempted to accomplish this, Janssen by giving a rotatory motion to a direct vision spectroscope, I by giving an oscillating motion to the slit, in which I was followed by Young, who afterwards expanded it.

"It occurred to me in connection with a suggestion of Professor Morton, that by interposing at the focus of the eye-piece a diaphragm which should move with the vibrating slit, the light of the neighbouring portions of the spectrum might be cut off and this dilution avoided. Mr. Clark has devised and constructed a very beautiful mechanical arrangement by which this simultaneous and accordant motion of slit and diaphragm is effected by the rotation of the small fly-wheel shown in Fig. 61 (p. 167).

"But I find that, although seen in this way, the prominences appear very bright; yet the working of the apparatus always causes a slight oscillation of the equatorial, which interferes with the definition of details.

* * * * * *

"But, although I do not use the vibration of the slit and diaphragm, I find the mobility of the slit so convenient as to be practically indispensable. In examining the spectrum of a group of sun-spots, for instance, it is very much easier to move the slit to the particular point we wish to observe, than to move the solar image by the tangent screws of the equatorial."[1]

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[1] See p. 483.

To Zöllner belongs the credit of having first indicated the open slit method now generally adopted.

This physicist, in a paper communicated to the Royal Saxon Academy of Sciences on February 6th, 1869,[2] makes the following remarks on the method of seeing the prominences without an eclipse.[3]

" . . . . . With a moveable slit, the brightness would be diminished in proportion to the distance travelled over by the slit. Especially in the rotating spectroscope the brightness of the protuberance itself would decrease from the centre of rotation to the edge, and thus prevent the observation of the natural brightness of its parts.

"For this reason I intend to employ another very simple arrangement. I have convinced myself of its practicability by the experiments on terrestrial sources of light described below.[4]

"The principles upon which it is based are as follows :—

"1. The apparent brightness (lustre, *claritas visa*) of a protuberance is independent of the opening of the slit, provided that it retains an appreciable breadth on the retina.

"2. The brightness of the superposed spectrum increases in proportion to the width of the slit.

"3. In oscillating or rotating slits the brightness of the superposed spectrum remains unchanged, while that of the image of the protuberance, produced by the permanence of the light, decreases according to another law, depending on the number and duration of the impressions on the retina, and on the refrangibility of the observed band of the protuberance.

"If, for simplicity, we suppose that the whole surface over which the slit travels in its rotation or oscillation is filled with the protuberance, and that the intensity of the produced secondary image is in inverse ratio to that surface (corresponding to a uniform diffusion of the light over that surface through a *stationary* slit), then the relation of the intensity of the background to that of the protuberance would remain the same whether we *first* decrease the brightness of the protuberance by means of *oscillation* of the slit, and thereby leave the brightness of the superposed spectrum or background (according to 2) unchanged,

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[1] *Nature*, December 8th, 1870. Extracted from the *Journal of the Franklin Institute.*

[2] Published in No. 1772 of the *Astronomische Nachrichten*, Sept 15th, 1860.

[3] The translation quoted is by Professor Mayer, and appeared in the *Journal of the Franklin Institute*, vol. lxxxviii. p. 410 et seq.

[4] These consisted in making the images of a petroleum lamp flame and that of a spirit lamp, the latter coloured with a salt, coincide on a reflector, and then observing and separating the combined images by means of the arrangement proposed.

NOTE D. or *secondly*, open the stationary slit so widely that the opening extends over the space travelled over by the oscillation of the slit in the first case. By this means (according to 1) the apparent brightness of the protuberance remains unchanged, while that of the background is increased in the same ratio in which it was diminished before with a constant background.

"If these suppositions are correct, the end in view can be much more easily obtained in the second way, if we take care not to admit the intense light of the body of the sun into the slit.

"It is only necessary to open the slit so far that the protuberance, or a portion of it, appears in the opening. By polarizing or absorbing media, placed before the eye-piece, the light in the whole field of view can be so diminished that the proper relation of intensity between the protuberance and the superposed spectrum may be obtained."

On the 16th of the same month in which Zöllner communicated his ideas to the Saxon Academy Mr. Huggins sent a paper to the Royal Society of London, in which he announces a method of observing the forms by means of a combination of an open slit and of ruby glass.[1]

"A spectroscope was used; a narrow slit was inserted after the train of prisms before the object-glass of the little telescope. This slit limited the light entering the telescope to that of the refrangibility of the part of the spectrum immediately about the bright line coincident with C.

"The slit of the spectroscope was then widened sufficiently to admit the form of the prominence being seen. The spectrum then became so impure that the prominence could not be distinguished.

"A great part of the light of the refrangibilities removed far from that of C was then absorbed by a piece of deep ruby glass; the prominence was then distinctly perceived . . . . . A more detailed account is not now given, as I think I shall be able to modify the method so as to make the outline of these objects more easily visible.

On the 29th I heard of this paper of Mr. Huggins', and it at once struck me that the absorptive media employed by him were useless, which is still my opinion, and I communicated this to the Royal Society on the 4th of March (see p. 482), proposing to use merely an open slit.

On the publication of Mr. Huggins' paper it was seen that his proposed arrangement set all optical principles at defiance, but I did not point this out until the publication by him, as notes to a translation of Dr. Schellen's work on Spectrum Analysis, first, a statement that the description given in the *Proceedings of the Royal Society* was erroneous [why had it not been corrected?], and secondly, that the method employed by him was identical with the one employed by Zöllner and myself![1]

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[1] *Pcoc. R.S.*, vol. xvii. p. 302.

Since this time Mr. Huggins has corrected his paper,[2] and I print the correction in a foot-note[3] that its value may be estimated, and that he may have the benefit of it so far as this work is concerned.[4]

In May 1869 Captain Herschel suggested the use of a prism of red glass[5] without a slit, and used the open slit method with success.

Whether the narrow or wide slit be used, it is important that the image should be enlarged by the spectroscope lenses and not by the object-glass of the telescope employed to throw the image on to the slit. This is accomplished by having the observing telescope longer than the collimator (see Fig. 85, and Zöllner, translated

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[1] "Schellen," pp. 423 and 425.

[2] *Proc. R.S.*, vol. xxi. p. 127.

[3] "When editing the English translation of Schellen's 'Spectrum Analysis,' I discovered that the short account of the method of viewing the forms of the solar prominences by means of a wide slit, which I had the honour of presenting to the Royal Society on February 16, 1869,* does not agree exactly in one respect with the account of the observations of February 13 as it was entered at the time in my Observatory-book. The short note was written at the suggestion of a friend during a Committee held in the Royal Society's apartments, and, as the concluding words show, was intended to be followed by a more detailed account of the method of observation. The point in question relates to the position of a second slit which was used to screen the eye from every part of the spectrum except that under observation. The words in my book written at the time are 'narrow slit found to be best at focus of little telescope with positive eyepiece.' In the note the second slit was stated to have been placed before the object-glass of the little telescope. Such an arrangement was tried in connection with some other experiments in progress at the time. The plan of limiting the field of view to the part of the spectrum corresponding to the refrangibility of the light of the prominence, as well as the employment of a ruby glass, is of value when the air is not favourable, or when a spectroscope of small dispersive power is used."

[4] The remarks of mine which called forth the correction will be found in *The Academy*, vol. iii. p. 290.

[5] *Proc. R.S.*, vol. xvii. p. 508.

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* *Proc. R.S.* vol. xvii. p. 302.

NOTE D.

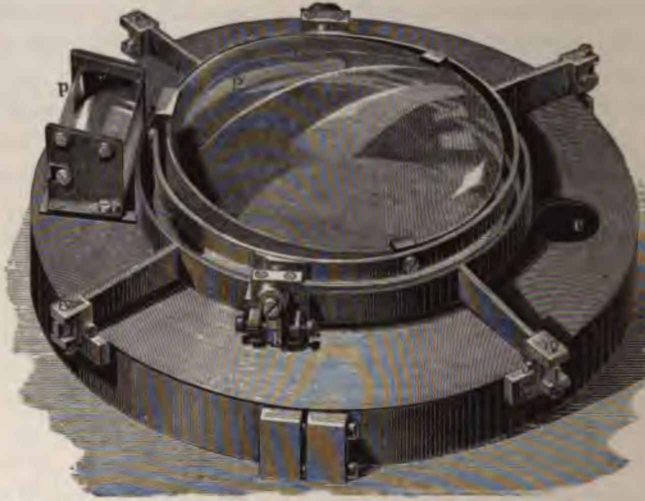

FIG. 161.—Cell with prism fixed. P is the prism, six Paris inches in diameter, mounted in a ring; its refracting angle is 12°; *a* and *a*, pins to attach the prism to the outer ring by means of the screws *b* and *b*; *c* is a hole to facilitate the finding of stars: P an achromatic system of prisms of the same refracting power as *p*: it is used in star-finding to overcome the inclination of the tube requisite when observing with P.

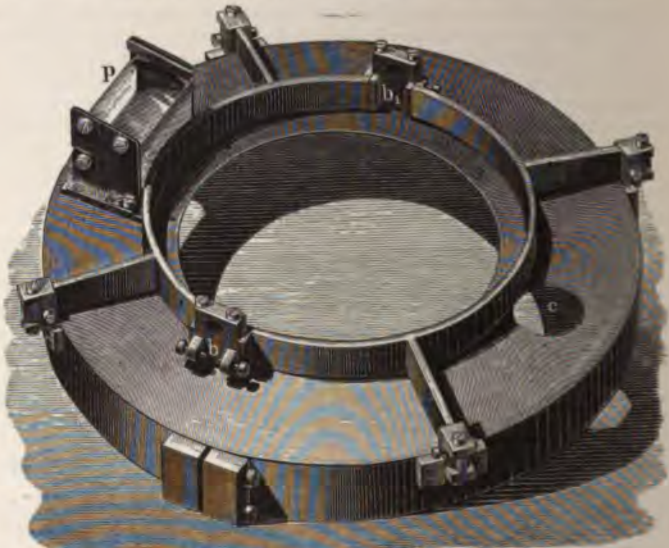

FIG. 162.—Cell without prism.

by Meyer, *Journal Franklin Institute*, vol. 88, p. 415, and Young, *Journal of the Franklin Institute*, No. 550, p. 348, Nov. 1871).

In addition to the methods above described, which all depend upon the use of the spectroscope in its ordinary form, others have been proposed. One method consists in placing a prism of small angle before the object-glass of the telescope, another in cutting the curves of an object-glass on the faces of a prism, another in placing a direct vision prism in front of the slit.

By means of these arrangements the slit of the spectroscope, situated in its ordinary position, could be immersed in light of any required refrangibility, the dispersion being increased to a large extent. In the case of eclipses it is of course possible to entirely do away with the slit, as the dark moon covers the sun's disc, while the corona is thus practically its own slit.

During the present year (1873), Mr. Seabroke and myself have communicated a method to the Royal Society which enables us to observe the whole of the chromosphere at once by means of a ring-slit :—[1]

"The observations made by slitless spectroscopes during the eclipse of Dec. 11, 1871, led one of us early this year to the conclusion that the most convenient and labour-saving contrivance for the daily observation of the chromosphere would be to photograph daily the image of a ring-slit, which should be coincident with an image of the chromosphere itself.

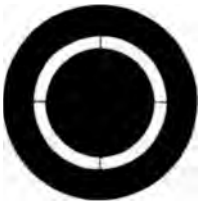

FIG. 163.—Diaphragm showing annulus, the breadth of which may be varied to suit the state of the air.

"The same idea has since occurred to the other.

"We therefore beg leave to send in a joint communication to the Royal Society on the subject, showing the manner in which this kind

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[1] *Proc. R.S.*, vol. xxi. p. 105. Read Nov. 6, 1872.

NOTE D. of observation can be carried out, remarking that, although the method still requires some instrumental details which will make its working more perfect, images of the chromosphere, almost in its entirety, have already been seen on several days during the present month and the latter part of last month.

"The adaptation of this method to a telespectroscope will be seen at a glance from the accompanying drawing.

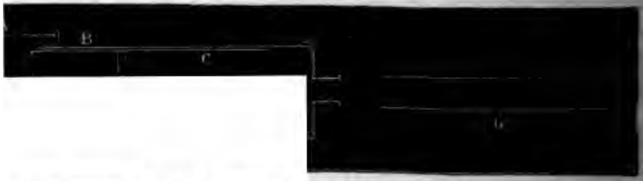


FIG. 164.—The annulus is viewed and brought to focus by looking through apertures in the side of the tubes. A. Sliding eye-tube of telescope. B. Tube screwing into eye-tube. C. Tube sliding inside B, and carrying lens D and diaphragm E. F. Lenses bringing image of diaphragm to a focus at the place generally occupied by the slit of the spectroscope. G. Collimator of spectroscope.

"The image of the sun is brought to focus on a diaphragm having a circular disc of brass (in the centre) of the same size as the sun's image, so that the sun's light is obstructed and the chromospheric light is allowed to pass. The chromosphere is afterwards brought to a focus again at the position usually occupied by the slit of the spectroscope; and in the eyepiece is seen the chromosphere in circles corresponding to the "C" or other lines. The lens D is used to reduce the size of the sun's image, and keep it of the same size as the diaphragm at different times of the year; and the lenses F are used in order to reduce the size of the annulus of light to about ⅛ inch, so that the pencils of light from either side of the annulus may not be too divergent to pass through the prisms at the same time, and that the image of the whole annulus may be seen at once. There are mechanical difficulties in producing a perfect annulus of the required size, so one an ½-inch in diameter is used, and can be reduced virtually to any size at pleasure.

"The proposed photographic arrangements are as follows:—

"A large Steinheil spectroscope is used, its usual slit being replaced by the ring one.

"A solar beam is thrown along the axis of the collimator by a heliostat, and the sun's image is brought to focus on the ring-slit by a 3¾-inch object-glass, the solar image being made to fit the slit by a suitable lens.

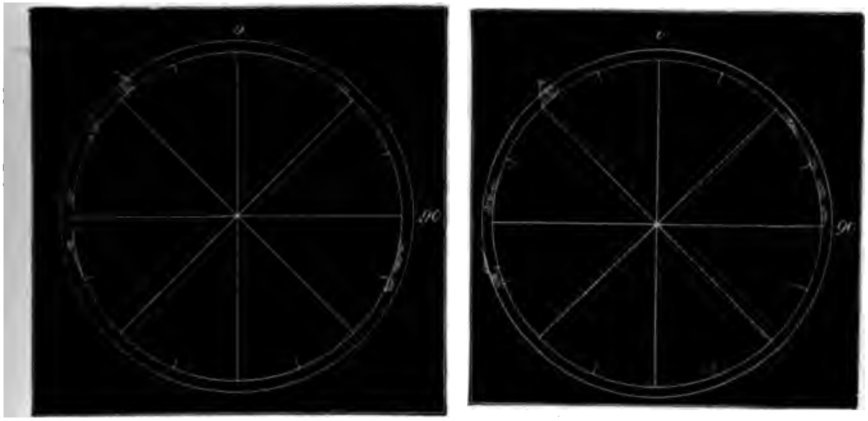
"By this method the image of the chromosphere received on the photographic plate can be obtained of a convenient size, as a telescope of any dimensions may be used for focusing the parallel beam which passes through the prisms on to the plate.

"The size of the image of the chromosphere obtained by the method adopted will be seen from the accompanying photograph, taken when the ring-slit was illuminated with the vapours of copper and cadmium.

"As this photograph is not reproduced, it may be stated that the ring images have an internal diameter of nearly ¾ of an inch."

NOTE D.

The accompanying solar profiles are copies of drawings made on the dates stated, by means of the new method, which were exhibited by the authors at the meeting :—


December 6, 1872, at 11.30.

December 7, 1872, at 11.30.

FIG. 165.—Outer circle 100″ from inner one. Chromosphere at normal height, except where prominences marked.

[Since reading the above paper, it has come to our knowledge that Zöllner had conceived the same idea unknown to us, but had rejected it. Professor Winlock, in America, has tried a similar arrangement, but without success.—J. N. L., G. M. S., January 17, 1873.]

NOTE E.

STRUCTURE, HEIGHT, AND COMPOSITION OF THE CHROMOSPHERE.[1]

NOTE E.

I add a note, and drawings of the edge of the chromosphere, entered in my Observatory-book on March 13, 1869 :—

FIG. 166.—Edge of chromosphere (billowy).

"Yesterday I watched the chromosphere by placing the slit at a tangent, and to-day got rather a different result. Yesterday I got a

NOTE E. billowy look. To-day, however, it was exactly like the edge of very ragged clouds."

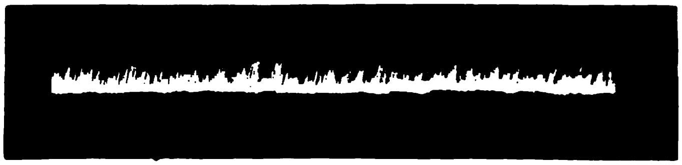


FIG. 167.—Edge of chromosphere (pointed).

The following are Professor Respighi's observations on this point, which entirely endorse my description at the page quoted:—

"In my Note III. at page 10, I said: 'The red stratum, or atmosphere of incandescent hydrogen, which envelopes the solar body, while appearing at its base to be regularly terminated in a circular arch, at the summit presents an irregular aspect, that is, at times terminating in points, or filaments either vertical or inclined, at times rounded like cloudy mounds, and finally undulating like mountain ranges. But very seldom (and this in the locality of the spots) it presents the appearance of long, regular strokes, even to the summit, resembling a bright circular arch.

"'The height of this stratum varies, but generally it does not exceed 12″. Its brilliancy also is variable in different parts of the solar outline, and at different times, and is generally greatest in the locality of the spots."

"' . . . . . The height of this stratum is variable in the several parts of the solar circumference, and generally seems higher in the vicinity of the poles than at the equator, and on rare occasions it is found to be very low near the large groups of protuberances.'"

On this point Secchi's statements are as follows:—

"Elle se présente sous quatre aspects bien tranchés.

"(*a*) Le premier aspect est celui d'une couche nettement terminée comme serait la surface libre d'un liquide. Son éclat tranche parfaitement à l'extérieur avec l'espace sombre environnant; on remarque seulement une faible diminution d'intensité près du bord extérieur.

"(*b*) Ordinairement, la chromosphère est garnie de petits filaments semblables à des poils brillants, dirigés dans un même sens, plus ou moins inclinés. Cette structure s'observe surtout entre les latitudes moyennes et les pôles. L'entraînement des filets n'est par toujours dirigé dans le sens des courants supérieurs, qui transportent les protubérances, mais cela arrive très-souvent.

"(*c*) Quelquefois, surtout dans les régions des facules, la surface est diffuse (*sfumata*), de manière qu'il est difficile de dire où elle s'arrête.

"(*d*) Enfin, le plus ordinairement, la chromosphère est terminée irrégulièrement et garnie de petits appendices coniques irréguliers, NOTE E.

ou de petites flammes dirigées en tous sens. Ce sont des protubérances rudimentaires, plus fréquentes dans les points du périmètre solaire où se presséntent les granulations ou marbrures de la surface; de sorte qu'il paraît exister une dépendance entre cet état de la chromosphère et les granulations.

"On pourrait distinguer ces quatre aspects de la chromosphère par les noms de *plate*, *velue*, *diffuse* (sfumata) et *flamboyante*."[1]

NOTE F.

PROMINENCES ON THE SUN.[2]

NOTE F.

I was at first under the impression that it was not probable that prominences would be seen on the sun itself (p. 479), and so was Father Secchi (*Comptes Rendus*, vol. lxviii. p. 237). We were, however, both mistaken.

My first observation of these was made on the 11th of April, 1869. Secchi's letter to the Paris Academy announcing the same appearance is dated the 13th, so that the observations must have been nearly simultaneous.

Father Secchi's first observations are thus recorded:—

"Mais ce qui faisait un étrange contraste avec cet aspect général de toutes les raies, c'était la raie C de l'hydrogène, qui s'effaçait complètement presque partout, et surtout dans les pénombres.

"J'ai mis la fente de l'instrument perpendiculaire au pont qui traversait la tache, et alors j'ai vu trois espèces de spectres bien tranchées dans le même champ de vision: 1° le spectre solaire ordinaire, 2° celui des taches avec les lignes noires et les bandes renfoncées; 3° les raies de l'hydrogène, disparues presque partout sur la pénombre, mais *devenues brillantes* sur le pont et sur la partie des noyaux la plus voisine de lui. Je ne saurais expliquer cet ensemble de phénomènes par de simples changements d'intensités: il y a évidemment ici une absorption très-puissante, et un renversement de raies, qui s'effectue manifestement aux points correspondants aux voiles rouges existant dans l'intérieur des noyaux. On a voulu, il est vrai, attribuer à une illusion d'optique ces voiles roses, mais il faudrait être bien mauvais observateur pour s'y méprendre: leur aspect contourné, en filets très-minces et entrelacés, suffit pour les distinguer des franges dues à l'aberration chromatique, et en faire reconnaître la réalité. Ces voiles ne sont donc autre chose que les protubérances rouges, comme je l'avais déjà dit: aujourd'hui nous avons en outre démonstration directe du renversement du spectre."[3]

---

[1] *Comptes Rendus*, vol. lxxiii. p. 827, October, 1871.

[2] See p. 488.

[3] *C. R.*, vol. lxviii. p. 960, 26th April, 1869.

NOTE F. The reference is to the former part of the same paper, where Secchi describes the appearance of the spot, in which these phenomena were observed, as seen by means of a polarizing eye-piece. He observes, "L'intérieur des noyaux était plein de voiles roses contournés et enchevêtrés de toutes manières" (p. 960).

Again :—

> "A 6 heures la raie C est très-forte partout sur le disque solaire, mais elle disparaît complètement dans le voisinage du noyau, sur les pénombres."[1]

Captain Herschel observed the bright lines of hydrogen on the sun's disc as early as June 10th, 1869.[2]

In a communication to the Franklin Institute, dated October 3, 1870, Professor Young states[3] that in a group of spots then visible, the C and F lines were frequently reversed. His account runs as follows :—

> "At 4.05 P.M. the brilliance of the F line increased so greatly that it occurred to me to widen the slit, and to my great delight I saw upon the disc of the sun itself a brilliant cloud, in all its structure and detail identical with the protuberances around the limb. Indeed, there were *two* of them, and there was no difficulty in tracing out and delineating their form. Fig. 168 represents them as they were from 4.05 to 4.10; Fig. 169 gives the form at 4.15-20. They were then considerably fainter than at first. During the intervening ten minutes I examined the other lines of the spectrum, and found that the form could be distinctly made out in all the *hydrogen* lines, even in *h*; but that the reversal of the other lines, including D_3, was confined to the region immediately over the spot-nucleus, where the smaller but brighter

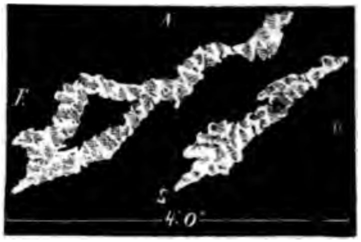


FIG. 168.—The prominence at 4.05.

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[1] *Comptes Rendus*, vol. lxviii. p. 1089, 10th May, 1869.
[2] *Proc. R.S.*, vol. xviii. p. 64.
[3] *Nature*, vol. iii. p. 113.

NOTE F.

cloud terminated abruptly; or, I might better say, *originated.* The larger one faded out at both ends. When the clockwork of the equatorial was stopped, the luminous cloud took 16·7 seconds of time to traverse the slit which was placed parallel to the hour-circle. This indicates a length of at least 130,000 miles, without allowing anything for the foreshortening resulting from the nearness of the sun's limb.

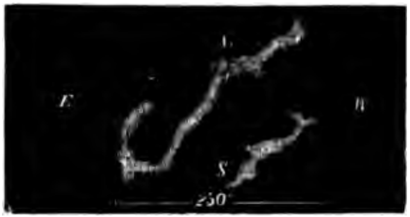

FIG. 169.—The prominence at 4.10.

> ". . . . . I may add that in the telescope this group of spots, from their first appearance, exhibited a strong yellowish tinge, which appeared to over-lie all the central portion of the cluster. So conspicuous was it, that several persons, unaccustomed to astronomical observation, noticed it at once before I called their attention to it. The penumbra of the group was unusually faint."

M. Rayet makes the following statement with regard to this subject:—

> "Mardi dernier, 12 Avril, 1870, en examinant au spectroscope la lumière d'une immense tache, centre d'un groupe très-étendu, situé dans la région sud-ouest du disque solaire, j'ai vu la ligne C se renverser et devenir lumineuse dans la portion répondant au noyau.
>
> "Je ne connais qu'une seule observation analogue: celle faite par le R. P. Secchi, le 12 Avril, 1869, et mentionnée en détail dans les *Comptes Rendus* du 6 Septembre, 1869.
>
> "Les circonstances que rendent possible le renversement des raies dans une tache sont donc peu fréquentes."[1]

NOTE G.

CHANGES OF WAVE-LENGTH.

NOTE G.

In no part of recent solar research (except in that relating to spots) has such disagreement occurred, both as to facts and the interpretation of them, as in relation to the

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[1] *Comptes Rendus*, vol. lxx. p. 846, 1870.

NOTE G. changes in position of the lines described by me immediately after the discovery of the new method, and ascribed, *on the ground of the selective nature of the phenomena*, to the approach or retreat of the vapours to which the particular lines in question are due.

Professor Respighi, whom I shall first quote, admitted from the first the reality of the phenomena, but objected to the explanation of them:—

> "Admitting that the refrangibility of the various luminous rays depends upon the length of the wave, as deduced by Fizeau, Billet, and others, then if the source moves rapidly towards the observer, the length of the wave is lessened, and the refrangibility increased; on the other hand, if the source of light moves rapidly from the observer, the refrangibility must be diminished, because the waves are lengthened.
>
> "The observations of the protuberances incontestably show that in some jets the hydrogen escapes from the sun with frightful velocity, at times not less than 500, 600, and even 700 kilomètres a second; and I myself have often observed this extraordinary velocity, especially in the jets near to the dark spots. As these jets are turned in all directions, it may therefore occasionally happen that on the solar circumference also there are similar jets found, either directed towards us, or in an opposite direction.
>
> "These phenomena, announced by Lockyer, Secchi, and others, were often observed by me, in every case in the jets near the locality of spots.
>
> "Portions of the chromosphere near spots are often observed to be peculiarly bright, and from them intensely bright and more or less violent jets are seen to arise. On placing the slit tangential to the limb, I have often observed at these points very sensible enlargements of the lines, especially of C, to which I have generally directed my attention. The line C was also occasionally seen to be irregular, being displaced towards the red or the violet, with a more or less sensible undulation.
>
> "After the development of the jets, on bringing the slit upon them, in parts (and this near to their base), the brightest streaks included by the slit, instead of appearing as lines, were presented occasionally in the form of lines broken at angles more or less obtuse. This appearance, however, comes out in a much less marked manner when the slit is narrowed; since then, those bright streaks were presented each time nearer to a general alignment with C. With the slit widened, it is often found that the line C of the red stratum, or of the protuberances, does not coincide exactly with the corresponding black line of the solar spectrum. Such phenomena are also observed in the case of F, and often on a larger scale. In the spectrum of the several parts of the solar disc, especially in the vicinity of the spots, I have often observed new formations and local enlargements of the dark line C, while in other tracts the line itself appeared remarkably lessened,

refined, or blended, and occasionally totally invisible, or transformed into a bright line. In these circumstances, the line C, losing its ordinary regularity of form, offered the aspect of an irregular line, more or less sensibly distorted. Arranging the slit perpendicularly to the solar outline in coincidence with the most intense jets, I have often observed the bright line C noticeably widened at the base of the jets, and not exactly coincident with the corresponding black line of the solar spectrum, and the streaks of jets comprised by the slit, arranged irregularly in relation to the same line, or not exactly coinciding with it; but even in this case, the irregularities diminish with the narrowing of the slit.

"Seeking the explanation of these phenomena, I have not considered it necessary to have recourse to the changes of refrangibility produced by the approach or the recession of the masses of hydrogen, in conformity with the principles above alluded to; but I have found, on the other hand, that at least the greater part of them could have been produced by other causes—namely, by the strong intensity of the light of the jets; by the influence of the opening of the slit; by the inexact accommodation of the slit plate to the focal distance of the instrument; from the state of agitation of the atmosphere, or from scintillation; and, finally, by the position of the eye in relation to the diaphragm, or to the axis of the eye-piece.

"The points, or bright streaks, which are present on the red stratum in proximity to the spots, and by which more or less gigantic jets are generally developed, are so brilliant, that they sometimes resemble images of the stars, and produce, on the outlines of the slit, bands of irradiation and of diffraction, through which the spectral lines appear more or less irregularly widened.

"When very fine and bright jets are to be seen, the irradiation and the diffraction, apparently lengthening their images, give to them the appearance of bright, irregular streaks, more or less inclined to the normal direction of the respective spectral lines. These appearances become very marked when the eye-piece is not exactly accommodated to the distance of distinct vision, and when the opening of the slit is not very small.

"When the slit is not very narrow, the atmospheric spectrum becomes diffuse, and on the dark zone corresponding to the black lines C and F, a line or black streak is obtained in a more marked way, which does not correspond to the central line of the slit; and therefore the bright lines of the chromosphere or of the protuberances not coming out symmetrically in relation to that false spectral dark line, appear to us more or less sensibly displaced from their normal position.

"If the eye-piece is looked through very obliquely to its axis, the very bright lines become easily diffused or doubled, in a way which seems as if, besides the normal spectral line, there must be a second one of greater or less refrangibility.

"When the atmosphere is much agitated, or when the scintillation is rather decided, the state of agitation of the bright lines and their irregular variations of intensity contribute to produce in them the illusion of greater or less irregularity in relation to the spectral lines.

"Finally, it is to be remarked that, to produce the above-mentioned

NOTE G. appearances, an actual widening of the spectral lines may greatly contribute (on account of the very elevated temperature at which we find the bases of the very luminous and violent jets).

"The irregularities which are presented in the lines C and F, in the spectrum of some parts of the solar disc, especially in the locality of spots, *i.e.* the enlargement of the lines themselves, their thinning, and lastly, their total disappearance or transformation into bright lines, are certainly due to the influence of jets or protuberances existing there.

"In the locality of the spots, the hydrogen proceeding from the violent and gigantic eruptions remains occasionally suspended for a long time at a great height, becoming invisible before returning to the sun, and forming real clouds of dark hydrogen. These clouds of hydrogen, acting then purely as a medium of absorption, may very well strengthen and widen the dark lines C and F in the subjacent parts of the photosphere.

"On the other hand, if the slit is placed upon a protuberance or a very brilliant jet, its brightness may weaken, or destroy, or neutralize the dark line; so that if the jet is sufficiently bright and the light of the photosphere sufficiently dim, the dark line may become bright—a state of things often observed in regions near the spots and in their penumbræ.

"I do not know whether these explanations may extend to all the phenomena observed by Lockyer, but I hold that with all methods they ought to be remembered before having recourse to a cause so uncertain at all times as that of the changes of refrangibility or the displacements of the spectral lines produced by the movement of the source of light.

"The question of the influence of the movement of the source of light on the length of the wave certainly is not so easy to decide as it has hitherto been thought; and therefore it is deserving of deeper study and discussion before we can accept as true the consequences above mentioned, and apply them to the explanation of these particular phenomena presented by the protuberances. We must ask for a rigorous demonstration of them, or a plausible confirmation, based upon direct and conclusive experiments.

"This question of optics has a certain analogy with that of the propagation of light through a refracting medium in movement; and while the first concerns the possibility of verifying the approach or the recession of the stars, and of the masses of incandescent hydrogen carried by the solar eruptions, by means of the changes of refrangibility, and the corresponding displacements of the spectral lines; the second concerns, on the contrary, the possibility of verifying or rendering sensible the movement of the refracting media, through which we observe the stars, by means of the differences between the aberrations corresponding to the various media traversed by luminous rays.

"In the year 1859, I have shown, by means of suitable experiments, that the aberration is independent of the medium through which we receive the luminous rays, and that therefore it is impossible to explain in this manner the movement of our refracting media, or the annual motion of translation of the earth, in which they necessarily participate.

> "Those differences of aberration resulted from the hypothesis that the rays or the luminous waves were propagated through the medium in motion, without suffering any influence, any deviation, or displacement of this movement. But as it has been demonstrated by experiment that these differences of aberration corresponding to the different methods do not take place, it must necessarily be concluded that they are compensated by a contrary effect produced on the light or luminous wave by the conditions of movement with which the medium by which they were promoted was invested.
>
> "The lengthenings and the shortenings of the luminous wave, by reason of removal or approach of the source of light, are deduced radically from the tacit supposition—from the hypothesis—namely, that the movement of the source of light, or of the centres of vibration, does not produce any other alteration in the conditions of the surrounding ether; and therefore the suspicion is not excluded, that in this case, also, no conditions of compensation can take place through which the luminous wave, in spite of the movement of its source, may arrive at the observer of its normal length—*i.e.*, that which it would have if the source were, in relation to the observer, in the condition of rest.
>
> "This question is of the greatest importance, and as it probably may not be definitively determined *a priori*—that is, by means of the theory, I hope it may come to be resolved *a posteriori*, by means, that is, of experiments more direct and more conclusive than those furnished by the solar eruptions."[1]

In his fifth note Professor Respighi seems to imply that he now admits the changes in the position of the lines as being independent of conditions. He writes:—

> "Under these masses, and above some jets emerging from them, I have often observed in the lines C, D_3, and F those enlargements and displacements brought to light principally by Lockyer and by Huggins, and which by them is attributed to changes of refrangibility due to the velocity or motion of the luminous gas in relation to the observer."[2]

It is therefore impossible to say whether he yet admits the explanation which ascribes these phenomena to motion.

The following are Father Secchi's observations on the subject. It will be seen that he from the first also admitted the facts, but suggested that the cause is the rotation of the sun, and not the motion of the gas as an independent

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[1] Respighi, Note iii. § xii. pp. 31—37. The references to Respighi's work throughout these notes are to his papers entitled "Sulle osservazione spettroscopiche del bordo e delle Protuberanze Solari fatte all' osservatorio dell' Università Romana sul Campedoglio." Extracted from the *Atti della Reale Accademia dei Lincei.* Note v., May 5, 1872.

[2] Respighi, Note v. § ix. p. 63.

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body. Speaking of a cloud visible above the solar limb on the 4th April, 1870, he writes:—

> "En la regardant avec l'image agrandie, on vit toute la ligne brillante rouge se projeter, non pas sur la raie noire de l'atmosphère extérieure au bord solaire, mais toute entière sur le côté lumineux plus réfrangible, laissant une ligne noire du côté du rouge extrême. C'était comme si la raie C eût eu une réfrangibilité plus grande que la raie noire qu'on voyait dans le champ extérieur au bord solaire.
>
> "Nous étions donc en face d'un de ces mystérieux changements de réfrangibilité qu'on a expliqués par le transport et par la force de projection de la matière solaire lancée avec une vitesse énorme.
>
> "Mais en réfléchissant bien cette fois, je conçus des doutes sur cette explication. En effet, pour produire le déplacement observé, qui etait au moins $\frac{1}{10}$ de la distance entre D′ et D″ du sodium, ce nuage aurait dû parcourir au moins 300 kilomètres par seconde; or, comme on ne voyait là aucun jet qui alimentât cette masse, comment imaginer une telle vitesse de projection? Et, de plus, comment concilier cette hypothèse avec la position relativement à la tache, qui était restée la même au moins pendant six heures? Pour admettre tout cela, il fallait admettre une foule de circonstances très-improbables, sinon impossibles, ce que exigeait au moins un extrême réserve.
>
> "Un peu de réflexion m'a montré qu'il n'y avait là autre chose qu'une conséquence nécessaire de la rotation du Soleil. En effet, cette astre, près de l'équateur, parcourt en un jour un angle de 864 minutes, c'est-à-dire 36 minutes par heure, ou 36 secondes dans une minute de temps, ce qui fait un arc de 0″, 6 dans une seconde de temps. Or une seconde d'arc soutend sur le soleil 716 kilomètres, de sorte que l'espace parcouru par un point équatorial dans une seconde de temps est 429 kilomètres."[1]

The next step in this question was taken by M. Fizeau, who, in a note communicated to the Academy on the 9th of May, 1870, pointed out that there were some very serious errors in Secchi's note of the 25th April, which he said demanded "*une rectification, tant à cause de l'importance de la question, que de l'autorité qui s'attache toujours aux communications de notre éminent correspondant.*"[2]

The next we hear of the matter comes from Secchi himself in the following letter:—

> "ROME, *ce 7 mai* 1870.
>
> "Je me hâte de rectifier une inexactitude qui s'est glissée dans ma dernière communication (*Comptes Rendus*, p. 905, ligne 10 en montant).
>
> "Il est dit qu'une seconde d'arc sur le Soleil soutend une longeur de 716 kilomètres; cela est vrai de la seconde géocentrique, ou vue

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[1] *Comptes Rendus*, vol. lxx. p. 905, 25th April, 1870.

[2] *Ibid*, vol. cit., p. 1013.

de la Terre, mais, dans le cas de la communication, on doit prendre la seconde héliocentrique, qui est de 3 km, 4; d'où résulte que l'arc parcouru en une seconde de temps correspond à l'étendue de 1 km. 92, ou presque 2 kilomètres. Cette diminution de la grandeur du mouvement ne détruit pas l'explication du phénomène dont il est question; au contraire, elle explique la petitesse du déplacement, car c'était pour moi une objection que de voir un déplacement si petit dans les raies, tandis que, avec le fort instrument que j'emploie, il devait être beaucoup plus sensible.

"Je vous prie d'insérer le plus tôt possible cette rectification dans les *Comptes Rendus*, même si quelque autre avait déjà relevé cette équivoque."[1]

It will be observed that it is now stated that the true motion of two kilometres per second accounts for the smallness of the displacement observed. In Secchi's first communication he had stated, that to produce the observed displacement, which was at least $\frac{1}{10}$th of the distance from D′ to D″, a motion of 300 kilometres per second was required.

One of his later statements to the French Academy on the question is the following:—

"J' ai vu aussi la dilatation de la raie C en forme de lozange telle que la décrit cette auteur [myself], et les distortions des lignes D_3 et F, que je désirais vivement vérifier."[2]

In the following quotation it will be perceived that he tacitly admits the justice of my explanation. The pamphlet from which it is taken is a reprint of a paper read by him before the Pontifical Academy Nuovi Lincei on the 7th of May, 1871. The italics are my own:—

"Vedendo la vivacita de' gette, restrinse la fessura, e trovai che la linea C si mostrava ondulata, *il che indicava una violenta translazione della massa.* Alla basa de' gette si vedevano molte altre righe, e specialmente la rossa a 0·45 di distanza da C verso B. Nel giallo D_3 si aveano dei getti, ma molto più bassi, e mancava la parte nebulosa. La nube era più viva, e più alta nel bleu, dove eguagliava almento il rosso. La linea viva F restava tutta intera dal lato meno refrangibile della nera corrispondente."[3]

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[1] *Comptes Rendus*, vol. lxx. p. 1062.

[2] *Ibid*, vol. lxxii. p. 306, March, 1871.

[3] "Sulle Protuberanze solari e le Facole." *Nota del P. A. Secchi*, 1871, pp. 5, 6.

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Finally, in November 1872, he fully accepts my original explanation:—

"Le P. Ferrari, mon assistant, en faisant le dessin de la belle tache qui était visible sur le disque, le 13 courant, à 10h. 45m. (et dont j'adresse une copie), s'aperçut qu'une langue très-vive de feu venait s'introduire au milieu du groupe des quatre noyaux principaux. La vivacité de sa lumière était telle, qu'elle surpassait, au moins du double, toute le reste du disque du Soleil et des facules environnantes. Malheureusement le ciel était semé de nuages, qui empêchaient un travail et une étude continus; mais pendant le temps qui fut employé à faire ce dessin, c'est-à-dire pendant 15 minutes environ, la langue changea de forme et prit l'aspect d'un globule très-brillant, qui paraissait soulevé du fond du disque solaire. Pendant cette transformation, il parut subir un faible déplacement.

"Aussitôt que je fus informé du phénomène, j'eus recours au spectroscope, et, le dirigeant sur la place indiquée, je constatai une éruption très-vive. L'hydrogène présentait ses raies renversées: la raie C était très-brillante, mais ce qu'il y a de plus remarquable, c'est que la partie la plus vive n'était pas en continuation avec la raie noire, mais s'en écartait obliquement comme dans la figure ci-contre où la partie ponctuée indique la portion renversée. Le déplacement était dans le sens de la réfrangibilité croissante. Ce phénomène démontrait la projection de la matière vers l'observateur."[1]

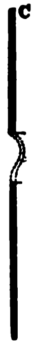

FIG. 170.

M. Rayet's ideas on this subject may be gathered from the accompanying extract. I do not know whether he has yet changed them, or if he still holds to these remarks printed in September, 1871:—

"Les lignes brillantes des protubérances, quelque soit leur réfrangibilité et leur degré d'intensité, se montrent *toujours*, si l'on observe avec un appareil bien réglé, *parfaitement droites et sans déviation aucune d'un côté ou de l'autre*. Il ne m'a jamais été donné de vérifier les phénomènes si bizarres de distortion et de déplacement que M. Lockyer signale dans plusieurs de ses communications, ou, du moins, toutes les fois qu'ils me sont apparus, j'ai pu constater la cause perturbatrice qui les produit.

"Comme M. Lockyer déduit de ces phénomènes des conséquences importantes sur la vitesse des courants gazeux de l'atmosphère solaire, il ne sera pas inutile de discuter ici les causes qui peuvent l'avoir induit en erreur.

"En premier lieu M. Lockyer signale que la raie F n'occupe pas exactement la position de la ligne noire de Fraunhofer, qu'elle est transportée d'une petite quantité vers la partie la moins réfrangible du spectre, et qu'elle est bordée d'une ligne sombre du côté de violet. Cette apparence est naturelle, car la ligne F est en réalité une ligne double formée de la ligne de l'hydrogène et d'une ligne un peu plus réfrangible du fer. Au bord du soleil la ligne F de l'hydrogène se renverse et la ligne du fer devient un peu diffuse, en sorte que l'intervalle que, sur la disque solaire, on peut distinguer entre les deux lignes, s'efface, et que F paraît bordé d'une bande noire du côté du violet. De cette apparence peut, je crois, naître l'idée d'un transport."

---

[1] *Comptes Rendus*, vol. lxxv. p. 1581, December 9th, 1872.

M. Rayet then refers to my announcements of the motion of the hydrogen masses in the sun (*Proc. Roy. Soc.* vol. xviii. p. 74), and says—" *De pareils observations, lorsqu' elles n'ont pu être vérifiées ni par le R. P. Secchi, ni par moi, peuvent-elles être admises, ou bien y a-t-il des motifs de les rejeter comme entachées d'erreurs?* " He then proceeds to treat the gaseous masses in motion in the sun by Bernouilli's formula for the flow of a gas into a vacuum and from the calculation obtains the following:—

" Vitesse d'écoulement de l'hydrogène dans le vide à la surface du soleil.

Température du gaz.	Vitesse par seconde.
0°	7892 mètres.
1000°	17036 „
2000°	21352 „
3000°	27315 „

" Les conditions d'écoulement d'un gaz à la surface du soleil sont certainement différentes de celles que suppose la formule de Bernouilli. Néanmoins, les nombres précédentes, plutôt trop forts que trop faibles, car, dans la realité, les gaz ne s'écoulent pas dans le vide, donnent, suivant une remarque de M. Wolf, une idée de la grandeur des vitesses avec quelles les gaz peuvent se mouvoir dans le Soleil, et on voit qu'il est difficile, impossible même, que cette vitesse dépasse 30 kilomètres par seconde."

The extreme rates observed by me on the sun are about 163 kilomètres per second. Young has seen greater velocities.

M. Rayet then proceeds to discuss the construction of my spectroscope:—

" Il resulte évidemment de cette construction que, pour regarder l'une ou l'autre extrémité du spectre, il faut placer la lunette obliquement par rapport à la direction des rayons lumineux dont on cherche l'image; or l'expérience journalière m'a prouvé que dans ces conditions de dissymétrie de la vision, le moindre défaut dans l'objectif ou dans l'oculaire se traduisait par un duplication des lignes brillantes ou par une distortion de ces lignes. Ces phénomènes singuliers disparaissaient

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d'ailleurs aussitot que l'appareil se trouvait dans les conditions normales.

"La fente dont les lèvres, loin d'être les intersections de deux plans, sont souvent deux surfaces cylindriques, peut aussi intervenir par doubles réflexions dans la duplication et le déplacement des raies.

"Enfin n'est-ce pas faire un abus des prismes que d'employer sept prismes, plus un prisme direct formé de cinq prismes alternes? est-il possible que ces-vingt quatre surfaces soient assez mathématiquement planés pour n'introduire dans la vision du spectre aucun trouble? Je ne le crois vraiment pas.

"C'est du reste l'opinion du R. P. Secchi que nombre des particularités vues par M. Lockyer seul peuvent être des erreurs d' observation. Voici comment s'exprime à ce sujet le savant directeur de l'Observatoire du Collége Romain:—'Il pourra bien se faire que tous ces mouvements et ces changements de réfrangibilité des raies, que M. Lockyer dit avoir observés, fussent des illusions. J'ai vu fréquemment des mouvements semblables se traduisant quelquefois par une duplication de la raie, mais je les ai attribués à l'agitation de notre atmosphère et à la chaleur solaire agissant sur la fente, qui peut bien produire des déviations accidentelles des rayons. Ordinairement ces phénomènes disparaissent en mettant bien au foyer l'appareil.'"[1]

The following are among Professor Young's observations as early as 1869 on the distortions and displacements of the various solar lines:—[2]

"While examining the spectrum of a large group of spots near the sun's western limb (on 4th September, 1869), my attention was drawn to a peculiar double *knobbiness* of the F line (on the sun's disc, not at the edge), represented by Fig. 170, *a*, at the point *e*.

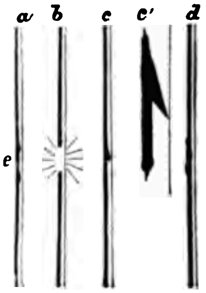

FIG. 171.—Young's first observations of alterations of wave-length.

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[1] "Memoire sur les raies brillantes du spectre de l'atmosphère solaire et sur la constitution physique du Soleil," pp. 26–31.—Thesis read before the Scientific Faculty of the University of Paris, 1871.

[2] *Journal of the Franklin Institute*, 1869, No. 526, p. 287. See also No. 528, p. 417.

In a very few moments, a brilliant spot replaced the knobs, not merely interrupting and reversing the dark line, but blazing like a star near the horizon, only with blue instead of red light; it remained for about two minutes, disappearing, unfortunately, while I was examining the sun's image upon the graduated screen at the slit, in order to fix its position, which was at—82½, about 43″ from the edge of the limb, about 15″ inside of the inner edge of the spot-cluster. I do not know, therefore, whether it disappeared instantaneously or gradually, but presume the latter. Fig. 171, *b*, attempts to give an idea of the appearance. When I returned to the eye-piece, I saw what is represented at Fig. 171, *c* and *c′*. On the upper (more refrangible) edge of F, there seemed to hang a little black mote, making a *barb*, whose point reached nearly to the faint iron line just above F. As given on Ångström's atlas, the wave-length of F is 486·07, while that of the iron line referred to is 485·92 (the units being millionths of a millimetre). This shows an absolute change of 0·15 in the wave-length, or a fraction of its whole amount, represented by the decimal ·00030, and would indicate an advancing velocity of about 55·5 miles per second in the mass of hydrogen whose absorption produced this barbed displacement.

"The barb continued visible for about five minutes, gradually resolving itself into three small lumps, one on the upper and two on the lower line, Fig. 171, *d*. In about ten minutes more, the F line resumed its usual appearance. I did not examine the C line, as I did not wish to disturb the adjustments, and risk losing some of the curious changes going on under my eye.

"After the close of this strange phenomenon, I examined, with our large telescope of 6-inches aperture, the neighbourhood in which this took place, and found a very small spot exceedingly close to, if not actually *at*, the place. This was at 2·45. At 5·30 it had grown considerably."

Not only have we an observation of a change of wave-length, but Professor Young's first glimpse of bright lines on the sun's disc.

A year later we have further evidence in this direction from the same observer:—

"Several spots have been carefully examined at different times: most of them, in their spectra, gave evidence of unusual disturbances; but by far the most interesting phenomena were exhibited by a large group which was first observed near the E. limb on September 19th, 1870. Changes of wave-length were frequent in its neighbourhood.

"Figs. 172 and 173 represent the appearances assumed by the F and C lines respectively, at the times indicated below each figure, during an observation on the afternoon of September 22nd, 1870. The point where these changes of wave-length occurred was at the western edge of the penumbra. At other times similar changes were observed, but not so great or rapidly varying."[1]

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[1] *Journal of the Franklin Institute*, and *Nature*, vol. iii. p. 112. See also *Journal of the Franklin Institute*, No. 535, p. 65, July, 1870.

NOTE G.

In the following extract Professor Young also notices the selective nature of these motions. He points out that

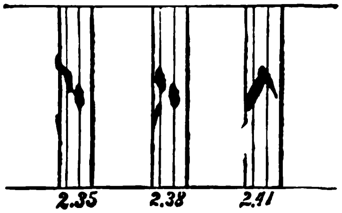

FIG. 172.—Changes in the F line.

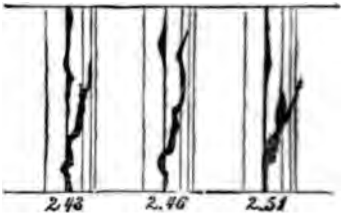

FIG. 173.—Changes in the C line.

b_3 (a nickel line), undergoes these changes, while b_1 b_2, and b_4 (magnesium lines), are at rest :—

"The C and F lines were reversed in some portion or other of the group nearly every time I observed it. On September 22nd, 1870, the *sodium* lines were both reversed for several hours, while D_3 appeared as a dark shade. On September 28th, again, at 4 P.M., the southern nucleus of the group (which at this time contained four large umbræ, besides many small ones) reversed all of the following lines, viz.: C ; D_1 ; D_2 ; D_3 ; 1474 ; b_1 ; b_2 ; b_3 ; b_4 ; F ; 2796 ; and *h*. All of these were *conspicuous*, except 1474 ; D_3 and b_3 especially so, and the latter (a nickel line) showed considerable changes of wave-length, alternate increase and diminution, which were not shared by its magnesium neighbours, b_1, b_2 and b_4."[1]

Professor Young finally states that on Mount Sherman

"Many more or less remarkable solar eruptions were observed, though none on quite so magnificent a scale as some before recorded. On several occasions velocities of from 150 to 200 miles per second in the ejected matter were observed by means of the displacement and distortion of the hydrogen lines, and on one occasion a velocity of nearly 250 miles was attained. One of the finest eruptions was visible on the surface of the sun itself in the immediate neighbourhood of a large spot."[2]

The following are Dr. H. C. Vogel's observations on this point : they were made in 1871 :—

"Die Protuberanz hatte sehr starke Veränderungen erlitten, Eine, längere Zeit isolirt schwebende, Wolke zeigte eine überaus starke rotatorische Bewegung. Die helle Wasserstofflinie H β erschien starke gekrümmt und gleichsam um die dunkle F Linie im Sonnenspectrum geschlungen, wie Abweichungen zu beiden Seiten der F Linie betrugen 0·23 Milliontel millimeter Wellenlänge, was einer Geschwindigkeit von Ca 20 Meilen in der secunde entspricht."

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[1] *Journal of the Franklin Institute*, October 3rd, 1870.
[2] *Nature*, vol. vii. p. 109.

Dr. Vogel has also seen and described that drift in two directions often seen in neighbouring spots:—

"Am 6 Mai untersuchten wir, Dr. Lohse und ich, spectroskopisch einen grossen Sonnenfleck, dessen Kern durch zwei Lichtbrücken gespalten war. Als der Spalt des Spectroskops auf diese Lichtbrücken gestellt wurde, zeigten sich Verschiebungen der Spectrallinien und zwar so, dass längs der Kante des grösseren Flecks die Linien mehr nach dem Violett, an der Kante des kleineren Flecks dagegen mehr nach dem Roth gerückt waren.

"Auffällig war es, dass sämmtliche Linien im Spectrum, so weit es untersucht werden konnte, an dieser Verschiebung theilnahmen, deren Grösse in der Nähe der F Linie zu 0·05 Milliontel Millimeter Wellenlänge geschätzt wurde.

"Die Geschwindigkeit des Emporsteigens der glühenden Gase am Rande des grösseren Flecks würde demnach 4 bis 5 Meilen in der secunde betragen haben."[1]

For the mathematical treatment of this subject see, in addition to Professor Clark Maxwell's paper referred to on page 202, Young in *Journal of the Franklin Institute*, No. 562, p. 349, November, 1872.

Professor Zöllner has devised a spectroscope, called by him a "Reversion Spectroscope," for the special purpose of observing these displacements of the spectral lines, and obtaining accurate measurements of their amounts.

This is effected as follows:—

"The line of light produced by a slit or by a cylindrical lens is in the focus of a lens, which makes the rays parallel as in other spectroscopes. Then the rays pass through two *amici's* systems of prisms, *à vision directe*, which I have received from Mr. Merz, Munich.

"These are so placed side by side that each gives passage to one half of the rays coming from the object-glass of the collimator, but so that the refracting edges lie on opposite sides. By this means all the rays are separated into two spectra of opposite direction. The object-glass of the telescope, which unites the rays again to one image, is cut perpendicular to the horizontal refracting edges of the prisms, as in the heliometer, so that each half can be micrometrically moved, either parallel or perpendicular to the line of bisection. By this means we are enabled not only to make the lines of both spectra coincide, but to place the two spectra side by side instead of superposing them (so that one is placed beside the other like a *nonius*) and also to partially

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[1] "Beobachtungen angestellt auf der Sternwarte des Kammerherrn von Bülow zu Bothkamp."—*Heft.* 1. 1872, p. 36 and 39.

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superpose them. By this construction the sensitive nature of the double image is not only turned to account for determining any change of position in the spectral lines, ***but every such change is also doubled*** because the influence acts in opposite directions in the two spectra.

"The principle of the ***reversion of the spectra*** which forms the basis of the instrument, and which determines me to give it the name of *Reversion Spectroscope*, may also be applied without the use of the *amici* system of prisms."[1]

NOTE H.[2]

LIST OF CHROMOSPHERIC LINES.

NOTE H.

My first list of lines[3] was, very shortly after it was published, confirmed by Professor Young, who added one more to it, 2581·5 (Kirchhoff).

> "I desire to call special attention to 2581·5, the only one of my list, by the way, which is not given on Mr. Lockyer's. This line, which was conspicuous at the eclipse of 1869, seems to be ***always present*** in the spectrum of the chromosphere, and shows the form of its upper surface or of a protuberance nearly as well, though of course not so brightly, as the 2796 line. It has no corresponding dark line in the ordinary solar spectrum, and not improbably may be due to the same substance that produces D_3.

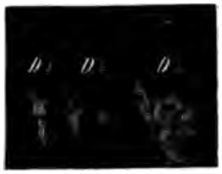

FIG. 174.—Prominence seen by D_1 D_2 and D_3 light.

> "The reversal of the sodium and magnesium lines is not at all uncommon. In some instances these lines were so bright, that on opening the slit, the form of the prominence could be made out through them. This was the case with a small hand-shaped prominence observed on September 27th. Comparing the form thus seen through D_1 and D_2 with that given by D_3, it appeared that the sodium

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[1] From Reports of Royal Saxon Academy of Sciences, Feb. 6, 1869, *Astr. Nacht.* No. 1772, Sept. 15, 1869. Translated by A. M. Mayer, Ph.D., in the *Journal of the Franklin Institute*, vol. lxxxviii., p. 405, Dec. 1869.

[2] See p. 491.

[3] Received by the Royal Society, July 8th, 1869.

line was sufficiently developed for observation only along the edge, and at one or two bright points in the prominence, most brilliantly neither at its summit nor its base. Fig. 174 represents the appearance (the slit was perpendicular to the sun's limb). The case was similar with the magnesium lines."[1]

Captain Herschel, in May 1869, communicated to the Royal Society a list of lines.[2]

Subsequently Young has continued the special study of this subject with the most magnificent success, and has published a first list of 103 lines, which has been followed by a second of no less than 273. He also endorses my observation, that at times every line appeared reversed. The preliminary catalogue sufficiently indicates the names of the other observers who have studied the question by the initials affixed to various lines showing by whom they were first observed.

The following is Professor Young's first catalogue,[3] obtained by observations made in September 1871. It includes only those lines which have been seen twice at least.

Ref. No.	Kirchhoff.	Ångström.	Relative Frequency.	Relative Brightness.	Chemical Element.	Previous Observer.
1	534·5	7060·?	60	3		
2	654·5	6677·?	8	4		L.
3	C	6561·8	100	100	H.	L. J.
4	719·0	6495·7	2	2	Ba.	
5	734·0	6454·5	2	3		
6	743·?	6431·	2	2		
7	768·?	6370·	2	2		
8	816·8	6260·3	1	1	Ti.	
9	820·0	6253·2	1	2	Fe.	

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[1] Communicated to the *Journal of the Franklin Institute*, October 3rd, 1870; *Nature*, vol. iii. p. 112. See also *Journal F.I.*, No. 524, p. 141, Sep. 1869.

[2] *Proc. R.S.*, vol. xvii. p. 507.

[3] Reprinted from the *American Journal of Science and Arts*, and *Nature*, vol. iii. p. 111.

NOTE H.

Ref. No.	Kirchhoff.	Ångström.	Relative Frequency.	Relative Brightness.	Chemical
10	874·2	6140·5	6	8	Ba
11	D_1	5894·8	10	10	N
12	D_2	5889·0	10	10	N
*13	1017·0	5871·	100	75	
14	1274·3	5534·0	6	8	Ba
15	1281·5	5526·0	1	1	Fe
16	1343·5	5454·5	1	2	Fe
17	1351·3	5445·9	1	2	Fe.
18	1363·1	5433·0	1	1	Fe
*19	1366·0	5430·0	2	3	
20	1372·0	5424·5	3	4	Ba
21	1378·5 ?	5418·0 ?	1	2	Ti.
*22	1382·5	5412·	1	1	
23	1391·2	5403·0	2	2	Fe.
24	1397·8	5396·2	1	2	Fe
25	1421·5	5370·4	1	2	Fe
26	1431·3	5360·6	2	2	
27	1454·7	5332·0	2	2	Ti
28	1462·9	5327·7	1	3	Fe
29	1463·4	5327·2	1	3	Fe
30	1465·0 ?	5321·	2	2	
	Corona line				
31	1474·1	5315·9	75	15	Fe.
32	1505·5	5283·	5	4	
33	1515·5	5275·0	7	5	
34	E_1	5269·5	1	3	Fe.
35	E_2	5268·5	1	2	Fe.
36	1528·0	5265·5	3	2	Fe.
37	1561·0	5239·0	1	1	Fe.
38	1564·1	5236·2	1	1	
39	1567·7	5233·5	2	2	Mn
40	1569·7	5232·0	1	2	Fe.
41	1577·3	5226·0	1	2	Fe.
42	1580·5 ?	5224·5	1	1	Ti.
43	1601·5	5207·3	3	3	Cr. F
44	1604·4	5205·3	3	3	Cr.
45	1606·5	5203·7	3	3	Cr. F
46	1609·3	5201·6	1	2	Fe.
47	1611·5	5199·5	1	1	
48	1615·6	5197·0	3	2	
49	b_1	5183·0	15	15	Mg
50	b_2	5172·0	15	15	Mg
51	b_3	5168·5	12	10	Ni.
52	b_4	5166·5	10	10	Mg
53	1673·9	5153·2	1	1	Na.
54	1678·0	5150·1	1	2	Fe.
55	1778·5	5077·8	1	1	Fe.
56	1866·8	5017·5	2	3	

Ref. No.	Kirchhoff.	Ångström.	Relative Frequency.	Relative Brightness.	Chemical Element.	Previous Observer.
57	1870·3	5015·?	2	2		R.
58	1989·5	4933·4	8	5	Ba.	L.
59	2001·5	4923·2	5	3	Fe.	R. L.
60	2003·2	4921·3	1	1		
61	2007·1	4918·1	3	3		L.
62	2031·0	4899·3	6	4	Ba.	L.
63	2051·5	4882·5	2	2		L.
64	F.	4860·6	100	75	H.	J. L.
65	2358·5	4629·0	1	1	Ti.	
66	2419·3	4583·5	1	1		
67	2435·5	4571·4	1	1	Li.	
68	2444·0	4564·6	1	1		
69	2446·6	4563·1	1	2	Ti.	
70	2457·8	4555·0	1	1	Ti.	
71	2461·2	4553·3	3	3	Ba.	
72	2467·7	4548·7	1	3	Ti.	
73	2486·8	4535·2	1	1	Ti. Ca. ?	
74	2489·5	4533·2	1	1	Fe.	
75	2490·6	4531·7	1	1	Ti.	
76	2502·5	4524·2	2	2	Ba.	
77	2505·8	4522·1	1	2	Ti.	
78	2537·3	4500·4	1	3	Ti.	
79	2553·?	4491·0?	1	1	Mn. ?	
80	2555·?	4489·5?	1	1	Mn. ?	
81	2566·5	4480·4	1	2	Mg.	L.
82	2581·5?	4471·4	75	8	A band rather than a line.	
83	2585·5	4468·6	1	1	Ti.	
84	2625·0	4443·0	1	1	Ti.	
85	2670·0	4414·6	1	1	Fe. Mn.	
86	2686·7	4404·3	1	2	Fe.	
87	2705·0	4393·5	3	2	Ti.	
88	2719·?	4384·8	1	1	Ca. ?	
89	2721·2	4382·7	1	2	Fe.	
90	2734·?	4372·	1	1		
91	2737·?	4369·3?	1	1	Cr.	
92	2775·8	4352·0	1	1	Fe. Cr.	
93	2796·0	4340·0	100	50	H.	L. J.
94	G.	4307·0	1	2	Fe. Ti. Ca.	
95	2870·0	4300·0	1	1	Ti.	
96		4297·5	1	1	Ti. Ca.	
97		4289·0	1	2	Cr.	
98		4274·5	1	2	Cr.	
99		4260·0	1	1	Fe.	
100		4245·2	1	1	Fe.	
101		4226·5	1	1	Ca.	
102		4215·5	1	2	Fe. Ca.	
103	*h.*	4101·2	100	20	H.	R. L.

NOTE H.

"In the table the first column contains simply the
number. An asterisk denotes that the line affected by it h
marked corresponding dark line in the ordinary solar spectr

"The second column gives the position of the line upon t
Kirchhoff's map—determined by direct comparison with t
the time of observation. In some cases an interrogatio
appended, which signifies not that the existence of the line i
but only that its precise place could not be determined, eith
it fell in a shading of fine lines, or because it could not be
the case of some close double lines which of the two comp
the bright one; or, finally, because there were no well-ma
lines near enough to furnish the basis of reference for a
accurate determination.

"The third column gives the position of the line upon
normal atlas of the solar spectrum. In this column an
interrogation mark denotes that there is some doubt as to t
point of Ångström's scale corresponding to Kirchhoff's.
considerable difference between the two maps, owing to th
of many faint lines by Ångström, and the want of the fine
of shading observed by Kirchhoff, which renders the co-ord
the two scales sometimes difficult, and makes the atlas of
far superior to the other for use in the observatory.

"The numbers in the fourth column are intended to deno
centage of frequency with which the corresponding lines
in my instrument. They are to be regarded as only roughl
mative; it would of course require a much longer period
vation to furnish results of this kind worthy of much confid

"In the fifth column the numbers denote the relative bri
the lines on a scale where 100 is the brightest and 1 th
These numbers also, like those in the preceding column, ar
to very little weight.

"The sixth column contains the symbols of the chemical s
to which, according to the maps above referred to, the lines
origin.

"There are no disagreeemnts between the two authorit
majority of cases, however, Ångström alone indicates the ele
there are several instances where the lines of more than one
coincide with each other and with a line of the solar spe
closely as to make it impossible to decide between them.

"In the seventh and last column the letters J., L., and R. de
to my knowledge the line indicated has been observed and
published by Janssen, Lockyer, or Rayet."

Professor Young called especial attention to t
numbered 1 and 82 in the catalogue; they are v
sistently present, though faint, and can be distinc
in the spectroscope to belong to the chromosphere
not being due, like most of the other lines, to the
tional elevation of matter to heights where it d

properly belong. He considers it to be probable that both these lines are due to the same substance which causes the D_3 line.

He also points out that the presence of titanium vapour in the prominences and chromosphere comes out very clearly from the catalogue, as no less than 20 of the whole 103 lines are due to this metal.

Young's second list,[1] obtained by observations at an elevation on the Rocky Mountains, is preceded by the following:—

TABLE showing the Number of Coincidences between the Bright Lines observed in the Spectrum of the Chromosphere, and those of the Spectra of the Chemical Elements.

				Unknown.	52	Total.
Fe. Ti. S($_w$)	1					
,, Ba. S($_w$)	1	Ti. S($_w$)	3	Fe.	64	110
,, S(w) Zn($_w$)	1	,, Ca.	2	Ti.	23	43
,, Co. Ce.	1	,, Mn	1	Ca.	10	29
,, Ni. E($_w$)	1	,, Ce.	1	Ba.	8	13
Ca. Cr. Ce.	1	,, Sr.	1	S($_w$)	7	14
,, Li. Zn.	1	,, Zn.	1	Mn.	6	12
Ti. Ba. S($_w$)	1	Ca. Cd.	1	Ce.	5	11
Ba. La. E($_w$)	1	,, Ce.	1	H.	4	4
Fe. Ca.	10	,, Co.	1	Na.	4	6
,, Ti.	9	,, Cr.	1	Cr.	4	10
,, Mn.	4	,, Sr.	1	Mg.	3	4
,, Cr.	3			Sr.	3	6
,, Ni.	3	S($_w$) E($_w$)	1	Zn.	3	9
,, Ba.	2			E($_w$)	2	9
,, Zn.	2	Mn. Zn.	1	Ni.	2	6
,, E($_w$)	2			Co.	1	5
		Cr. E($_w$)	1	Cu.	1	2
,, Ce.	1			La.	1	3
,, Co.	1	Ce. Co.	1	Ru. Ir.	1	1
,, Mg.	1			Cd.		1
,, Na.	1	Na. Cu	1	Li.		1
,, S($_w$)	1					
,, La.	1	lines marked with an *	14			

The numbers in the last column denote the whole number of times that the symbol of each element appears in the catalogue, either singly or combined with others.

He states :—

"The great altitude of the station (nearly 8,300 feet), and the consequent atmospheric conditions, were attended with even greater

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[1] *Nature*, vol. vii. p. 17.

NOTE H. advantages for my special work than had been really expected, although I was never quite able to realize my *hope* of seeing all the Fraunhofer lines reversed, unless once or twice for a moment, during some unusual disturbances of the solar surface.

"Everything I saw, however, confirmed my belief that the origin of the dark lines is at the base of the chromosphere, and that the ability to see them all reversed at any moment depends merely upon instrumental power and atmospheric conditions."

CATALOGUE OF BRIGHT LINES IN THE SPECTRUM OF THE CHROMOSPHERE. 1872.

No.	P.C.	K.	A.	F.	B.	E.
1‡	1†	534·0*	7055·?	100	12	
2	2†	654·3	6676·9	25	50	Fe. Ba($_w$)
3	3†	C.694·1	6561·8	100	100	H.
4		711·4	6515·5	15	4	
5	4	718·7	6496·0	18	5	Ba.
6		731·7	6461·7	5	2	Ca.
7	5	734·0	6453·8	10	6	
8		740·9	6438·1	5	2	Ca. Cd.
9‡	6	744·3	6429·9	20	4	
10		750·1	6415·6	5	2	
11		756·9	6399·0	5	2	Fe.
12		759·3	6392·6	5	1	Fe.
13		767·?*	6373·?	5	2	
14	7	768·?*	6371·?	5	3	
15		778·3	6346·1	10	4	Ruth. Ir.
16‡	8	823·5	6245·4	8	5	Fe.
17‡	9	827·6	6237·3	8	2	
18		830·2	6231·5	5	1	Fe.
19		836·5	6218·3	3	1	Ti.
20		839·2	6214·1	3	1	Ti.
21		845·7	6199·6	2	2	Fe.
22		849·7	6190·5	10	2	Fe.
23		859·7	6168·3	3	1	Ca.
24		863·9	6161·2	8	3	Ca.
25		⎰ 870·9	6148·1	3	2	Fe. E($_w$)
26		⎱ 871·4	6146·8	3	2	
27	10	874·3	6140·6	25	10	Ba.
28		⎰ 876·5	6136·1	2	1	
29		⎱ 877·0	6135·6	2	1	Fe.
30		884·9	6121·2	5	3	Ca. Co.
31		890·2	6109·9	2	1	Ba.
32		894·9	6101·7	3	2	Ca. Li. Zn($_w$)
33		903·1	6083·1	3	2	Ti.
34		912·1	6064·5	5	2	Fe. Ti.
35		933·8	6018·0	2	1	Ba.
36		949·4	5990·0	10	4	
37		992·0	5913·2	2	1	Fe.
38	11†	$D_1$1002·8	5895·0	50	30	Na.
39	12†	$D_2$1006·8	5889·0	50	30	Na.
40‡	†	1011·2	5883·0	2.	1	Fe.

No.	P.C.	K.	A.	F.	B.	E.
41‡	13†	$D_3$1016·5*	5874·9	100	90	
42		1031·8	5852·7	8	2	Ba.
43		1135·1	5708·3	1	1	Fe.
44		1151·1	5687·2	2	1	Na.
45		1154·2	5683·5	5	3	
46		1155·8	5681·5	2	1	Na. Fe. N($_w$)
47		1165·7	5667·8	2	2	S($_w$)
48		1167·0	5666·0	1	1	
49		1170·6	5661·5	15	2	Fe. Ti. E($_w$)
50		1175·0	5656·7	8	3	S($_w$) N($_w$)
51		1176·6	5654·4	2	1	Fe.
52		1187·1	5640·2	1	1	S($_w$)
53		1189·3	5637·3	1	1	
54		1200·6	5623·2	2	1	Fe.
55		1207·3	5614·5	2	1	Fe.
56		1229·6	5587·6	2	2	Ca.
57		1231·3	5585·5	2	1	Fe.
58	14†	1274·2	5534·1	50	12	Ba. Fe. Sr.
59	15	1281·3	5525·9	40	5	Fe.
60		1287·5	5518·7	15	2	Ba.
61		1298·9	5505·8	2	1	Fe.
62		1303·5	5500·5	2	1	Fe. La.
63		1306·7	5496·6	2	1	Fe. E($_w$)
64		1320·6	5480·2	2	1	Ti. Sr.
65		1324·8	5475·9	1	1	Ni.
66		1328·7	5472·3	3	1	
67		1337·0	5462·3	1	1	Fe. N($_w$)
68	16	1343·5	5454·7	10	4	Fe.
69	17	1351·1	5445·9	10	4	Fe. Ti. Br($_w$)
70		1360·9	5435·4	5	2	Zn Br($_w$)
71		1362·9	5433·0	2	2	Fe.
72‡	18	1364·3	5431·8	8	5	
73	19	1367·0	5428·8	8	3	Fe. Ti.
74	20	1372·1	5424·5	25	6	Ba. Ti. S($_w$)
75	21	1377·4	5417·9	5	2	Ti. Mn.
76		1380·5	5414·5	2	2	Fe.
77	22	1382·5	5412·4	4	2	Mn($_w$)
78		1384·7	5410·0	2	1	Fe. Ni.
79		1385·7	5409·0	2	2	Cr.
80		1389·4	5404·8	2	1	Fe.
81	23	1390·9	5403·1	5	3	Fe. Ti.
82		1394·2	5399·6	2	1	Mn.
83	24	1397·5	5396·1	4	2	Fe. Ti.
84		1401·6	5392·2	2	1	Fe. Ce.
85		1412·5	5380·2	3	2	Ti.
86	25†	1421·5	5370·5	10	3	Fe.
87		1423·0	5369·0	1	1	Fe.
88		1425·4	5366·5	1	1	Fe.
89		1428·2	5364·0	1	1	Fe.
90	26†	1430·1	5361·9	20	10	Fe.
91		1438·9	5352·4	4	2	Fe. Co. Ce.
92		1446·7	5345·0	1	1	
93		1450·8	5340·2	1	2	Fe. Mn. O($_w$)

R R

NOTE H.

No.	P.C.	K.	A.
94	27	1454·7	5335·9
95		1461·5	5329·1
96	28	1462·8	5327·1
97	29	1463·3	5327·6
98		1464·8	5325·1
99		1471·9*	5318·0
100‡	31†	1473·9	5315·9
101		1476·8	5313·1
102		1497·3	5292·0
103	32	1505·3	5283·4
104	33†	1515·5	5275·0
105	34	E_1 1522·7	5269·5
106	35	E_2 1523·7	5268·5
107	36†	1527·7	5265·8
108		1530·2	5263·3
109		1538·5*	5256·2
110		1541·9	5254·1
111		1547·7	5249·7
112		1551·6	5246·3
113	37	1561·0	5239·0
114	38	1564·2	5236·3
115	39†	1567·5	5233·6
116	40	1569·6	5232·1
117		1575·4	5227·5
118	41	1577·4	5226·2
119		1578·1	5225·5
120	42	1580·1	5224·3
121		1589·1	5216·5
122		1590·7	5215·5
123		1592·3	5214·4
124		1597·9*	5210·5
125		1598·9	5209·5
126	43	1601·5	5207·6
127	44	1604·4	5205·2
128	45	1606·4	5203·7
129	46	1609·2	5201·5
130	47	1611·3	5199·7
131		1613·9	5197·9
132	48†	1615·6	5197·0
133		1617·4	5195·0
134		1618·9	5194·1
135		1627·2	5188·2
136		1628·2	5187·3
137		1631·5	5185·1
138	49†	b_1 1634·1	5183·0
139	50†	b_2 1648·8	5172·0
140	51†	b_3 1653·7	5168·3
141	52†	b_4 1655·6	5166·7
142		1666·?	5160·?
143		1671·5	5154·8
144	53	1673·7	5152·5
145	54	1677·9	5150·1
146		1689·5	5142·2

No.	P.C.	K.	A.	F.	B.	E.
147		1701·8	5133·0	1	1	Fe.
148		1704·7	5130·8	1	1	Fe.
149		1707·9	5128·6	1	1	Ti.
150		1710·7	5126·7	1	1	Fe. Ti.
151		1712·2	5125·5	1	2	
152‡		1713·4	5124·4	1	1	Fe.
153		1715·2	5123·2	1	1	Fe.
154		1717·9	5121·0	1	1	Fe.
155		1719·4	5119·9	1	1	Ti.
156‡		1727·3	5114·9	1	1	Ni.
157		1734·6	5108·8	2	2	Ti($_w$)
158		1737·7	5107·0	1	1	Fe.
159		1750·4	5098·1	1	1	Fe.
160		1752·8	5096·5	1	1	Fe. S($_w$)
161		1765·0*	5087·0	2	1	E($_w$)
162		1771·5	5083·5	1	1	Zn($_w$)
163	55	1778·5	5077·9	1	2	Fe.
164		1823·6	5047·8	2	2	Fe.? Zn($_w$)
165		1833·4	5041·2	2	2	Fe. Ca.
166		1834·3	5040·1	2	2	Fe.
167		1848·9	5030·1	4	3	S($_w$)
168		1856·9	5023·5	3	1	S($_w$)
169	56†	1867·1	5017·6	30	15	Fe. Ni.
170	57†	1870·6	5015·0	30	10	Ti($_w$)
171		1905·1	4993·3	2	1	Fe. N($_w$)
172‡		c.1961·0	4956·7	1	2	Fe.
173	58†	1989·5	4933·4	30	8	Ba.
174	59†	2001·6	4923·1	40	12	Fe. S($_w$) Zn($_w$)
175	60†	2003·2	4921·3	30	8	S($_w$)
176	61	2007·2	4918·2	20	3	Fe.
177		2016·0	4911·2	3	2	Zn($_w$)
178	62†	2031·1	4899·3	30	6	Ba. La. E($_w$)
179	63†	2052·5?*	4882·9	10	4	Ce.
180		2067·8	4869·4	5	1	
181	64†	F 2080·0	4860·6	100	80	H.
182		2087·6	4854·7	5	2	Fe. Ni. E($_w$)
183		2094·0	4848·1	3	2	Ca. O($_w$)
184		2116·?*	4826·5	1	1	
185		2121·2	4822·8	10	2	Mn.
186		2142·4	4804·4	3	1	Ti. S($_w$) O($_w$)
187		2171·5	4778·7	3	2	Co. N($_w$)
188		2229·1	4730·8	1	1	Fe.
189‡		2251·3*	4712·5	2	2	Ce. O($_w$)
190		2309·5	4666·3	3	1	Fe. Ti.
191		2314·3	4663·3	2	1	
192		2323·0	4656·0	2	1	Ti.
193	65	2358·4	4629·0	15	8	Ti. N($_w$)
194		2359·5*	4628·2	2	1	Ce.
195		2369·7	4620·3	1	1	
196		2410·2	4589·4	1	1	
197		2412·8	4587·5	2	2	
198	66	2419·3	4583·2	15	6	
199		2429·5	4576·0	4	2	

NOTE H.

No.	P.C.	K.	A.	F.	B.	
200	67	2435·5	4571·4	10	4	
201	68	2443·9	4564·8	10	3	
202	69	2446·6	4563·2	10	5	
203		2452·1	4559·5	8	2	
204		2454·1	4558·1	8	1	
205	70	2457·9	4555·3	10	5	
206	71	2461·2	4553·4	10	5	
207		2463·4	4551·8	1	1	
208	72	2467·6	4548·9	10	8	
209		2480·8	4539·2	2	1	
210	73	2486·6	4535·5	2	2	
211	74	2489·4	4533·2	5	5	
212		2490·5	4532·1	3	2	
213	76	2502·2	4524·4	3	2	
214	77	2505·6	4522·0	3	3	
215		2517·0	4514·0	2	1	
216		2518·4	4513·0	1	1	
217		2527·0	4506·0	2	1	
218	78	2537·1	4500·3	15	6	
219	79	2552·4	4490·9	20	8	
220	80	2555·0	4489·4	15	3	
221	81	2566·3	4480·9	5	2	
222‡	82†	f 2581·2	4471·2	100	25	
223	83	2585·4	4468·5	20	5	
224		2620·8	4446·3	1	1	
225	84	2625·2	4443·0	10	2	
226		2633·0	4436·7	1	1	
227		2639·6	4433·5	1	1	
228		2651·5*	4426·0	2	3	
229		2653·2	4425·0	2	2	
230		2664·9	4418·0	2	1	
231		2665·9	4417·5	3	1	
232	85	2670·0	4414·7	1	1	F
233		2680·0	4407·7	1	1	
234		2686·8	4404·2	1	1	
235		2696·0	4398·5	1	1	T
236		2698·2	4396·5	1	2	
237	87	2702·5	4394·6	15	3	
238		2715·2	4388·5	1	1	
239	88	2718·5	4384·7	8	2	
240		2720·2	4383·5	1	1	
241	89	2721·6	4382·8	1	1	
242		2725·8	4380·4	1	1	
243		2728·0	4379·1	1	1	
244	90	2733·7	4375·5	5	3	
245	91	2736·9	4374·2	8	3	
246		2762·0	4359·1	1	1	
247	92	2775·7	4351·8	3	1	
248	93†	2795·7	4340·1	100	65	
249		2798·0	4338·2	10	2	
250		2805·4	4335·1	2	1	
251		2823·4	4324·0	1	2	
252		2830·7	4320·1	1	1	

No.	P.C.	K.	A.	F.	B.	E.
253		2843·0	4313·5	1	1	Ti.
254	94	G.2854·2	4307·2	3	2	Ca. Fe.
255	95	2867·7	4302·1	3	2	Ca. Fe.
256	96	2874·2	4298·0	1	1	Ca. Fe.
257	97	2894·5	4289·4	1	1	Cr. Ca. Ce$(_{w})$
258	98	2928·5	4274·6	2	1	Cr. Ca.
259	99	8961·2	4260·0	2	1	Fe.
260	100	2996·2	4245·2	30	3	Fe.
261		3018·0	4235·5	30	5	Fe.
262		3022·8	4233·0	15	5	Fe. Ca.
263	101	3040·0	4226·3	3	3	Ca. Sr.
264	102	3061·8	4215·3	40	7	Ca. Sr.
265		3155·5	4178·8	1	1	
266		3187·0	4166·7	1	1	Ca.
267	103†	h.3363·5	4101·2	100	50	H.
268		3431·0	4077·0	25	2	Ca.
269		3526·0	4045·0	3	2	Fe.
270		3703·3	3990·?	2	1	
271		3769·5	3970·?	2	1	Fe.
272‡		$\mathrm{H}_1$3778·5	3967·9	75	3	Fe. Ca.
273		$\mathrm{H}_2$3882·5	3932·8	50	1	Fe. Ca.

NOTES.

1. The position assigned to this line, first observed by Respighi (a fact of which I was ignorant when the Preliminary Catalogue was published), rests upon two series of micrometric measurements, referring it to four neighbouring dark lines—the probable error is about $\frac{1}{20}$th of a division of Kirchhoff's scale.

9. No. 6 in P.C. Position there given, 743?

16 and 17. Nos. 8 and 9 of P.C. Position given as 816·8 and 827·6, by a mistake in identifying lines upon the map.

40. I have never myself seen this line reversed. Prof. Emerson, however, saw it several times. It was first reported by Rev. S. J. Perry, in *Nature*, vol. iii. p. 67.

41. The position of this line has been independently determined by three series of micrometric comparisons with neighbouring lines. My result agrees exactly with that of Huggins.

72. Erroneously given in P.C. as 1363·1, which line does not reverse, or at least was never seen reversed at Sherman.

100. The principal line in the spectrum of the corona. The corresponding line in the spectrum of iron is feeble, and on several occasions when the neighbouring lines of iron (1463, &c.) have been greatly disturbed, this has wholly failed to sympathise. Hence I have marked the Fe with a ?. Watts indicates a strong line of oxygen at 5315 A.

152 and 156. Observed only on one day, but verified by Prof. Emerson.

172. Called "little C" by Mr. Stoney.

179. Given by Lockyer as K 2054. Its position is a little uncertain ; it seems to coincide with neither of the dark lines at 2051 and 2054, but lies between them, a little nearer to 2051.

189. Rather a band than a line.

222. The position of this line, which, however, like 189, is rather a band,

NOTE H.

was determined by two series of careful micrometrical measuremen
discovered by Rayet in January, 1869; since called "f" by Lorenz
272 and 273. These lines were both reversed (by a narrow br
running down the centre of the broad hazy band) as constantly, wh
seeing was good, as *h* or C itself. The observation was difficult, ho
required the most scrupulous exclusion of foreign light, and a care
ment of the slit in the plane of the solar image formed by these
rays.

They were also found to be regularly reversed upon the body
itself, in the *penumbra and immediate neighbourhood of every importa*

"In the catalogue, the first column contains simply a
number: a ‡ refers to a note at the end of the catalogue.

"The numbers in the second column refer to the 'Pr
Catalogue,' containing 103 lines, which was published a ye
the *American Journal of Science.* In this column a †
that some other observer has anticipated me in the determin
publication of the line. As I have depended for my inf
almost solely upon the *Comptes Rendus* and the *Proceedin*
Royal Society (which gave the observations of Lockyer,
Rayet, and Secchi), it is quite possible that some other lines
be marked in the same manner.

"The third column, headed K, gives the positions of the
Kirchhoff's scale, the numbers above G being derived from
continuation of Kirchhoff's maps. In this column an asterisk
that the map shows no corresponding dark line, a ? that
position, not the existence, of the line is for some reason
uncertain.

"The fourth column, headed A, gives the wave-length of th
ten-millionths of a millimeter, according to Ångström's atlas.

"The numbers in this and the preceding column were ta
from the maps themselves, which present slight inaccur
account of the shrinking and swelling of the paper during th
tion of printing, but from the numerical catalogues of Kirch
Ångström which accompany their respective atlases. In the
nary Catalogue the numbers were derived from the maps; her
slight discrepancies in the tenths of division.

"The fifth column, marked F, contains a rough estimate of
centage of frequency with which the lines were seen during
weeks of observation; and the sixth column, B, a similar est
their maximum brightness compared with that of the hydroge

"The variations of brilliance, however, when the chromosp
much disturbed, were so considerable and so sudden that
great weight can be assigned to the numbers given. Nor is
inferred that lines which have in the table the same index o
ness were always equally bright. On certain occasions on
lines would be particularly conspicuous; on others, another.

"With two or three exceptions, indicated in the notes,
have been catalogued which were not seen on at least two
days. In the few cases where lines observed only on one
have been admitted to the list, the observations were at
carefully verified by my assistant, Prof. Emerson, so as to pl
correctness beyond a doubt. Many other lines were 'glim

one time and another, but not seen steadily enough or long enough to admit of satisfactory determination.

"The last column of the catalogue contains the symbols of the chemical elements corresponding to the respective lines. The materials at my disposal are the maps of Kirchhoff and Ångström, Thalén's map of the portion of the solar spectrum above G, and Watts' "Index of Spectra." Since the positions of the lines in the latter work are given only to the nearest unit of 'Ångström's scale,' I have marked the coincidences indicated by it with a (w), considering them less certain than those shown by the maps."

Professor Young thus sums up his work:—

"In addition to the elements before demonstrated to exist in the chromosphere, the following seem to be pretty positively indicated—sulphur, cerium, and strontium; and the following with a somewhat less degree of probability, zinc, erbium and yttrium, lanthanum and didymium. There are some coincidences also with the spectra of oxygen, nitrogen, and bromine, but not enough, considering the total number of lines in the spectra of these elements, or of a character, to warrant any conclusion. One line points to the presence of iridium or ruthenium, and only three are known in the whole spectrum of these metals.

"No one, of course, can fail to be struck with the number of cases in which lines have associated with them the symbols of two or more elements. The coincidences are too many and too close to be all the result of accident, as for instance in the case of iron and calcium, or iron and titanium.

"Two explanations suggest themselves. The first, which seems rather the most probable, is that the metals operated upon by the observer who mapped their spectra were not absolutely pure—either the iron contained traces of calcium and titanium, or *vice versâ*. If this supposition is excluded, then we seem to be driven to the conclusion that there is some such similarity between the molecules of the different metals as renders them susceptible of certain synchronous periods of vibrations—a resemblance, as regards the manner in which the molecules are built up out of the constituent atoms, sufficient to establish between them an important physical (and probably chemical) relationship.

"I have prefixed to the catalogue a table showing the number of lines of each substance, or combination of substances, observed in the chromosphere spectrum; omitting, however, oxygen, nitrogen, and bromine, since with one exception (line 230) neither of them ever stands alone, or accounts for any lines not otherwise explained."

Quite recently Professor Young has shown the possibility of distinguishing between those lines which are always reversed in the chromosphere and those which, coming from greater depths, are thrown into it by uprushes.

"I must not close without alluding to certain observations that enable us to distinguish, to some extent, between the substances

NOTE H.

ejected from the sun, and those constituting the atmosphere
the irruption takes place. Certain lines during these out
distorted and displaced, while others near them, equally c
were wholly unaffected.

"Thus on August 3rd and 5th, the former class included
hydrogen, D_3, the lines of sodium, magnesium, and many
iron; in the latter were K534, 1474, 1505, 1515, 1528, 1867,
and 200 were intensely disturbed), 2581, and probably the
say probably, because the observation of these lines was t
to permit absolute certainty, still I feel very confident that
unaffected. The barium lines also seldom seemed to pa
any disturbance.

"The obvious moral of our summer's work seems to me t
time ought to be lost in occupying points of such advanta
most powerful instruments: the great telescopes now build
be put in a position to profit by such atmospheric conditi
secure their utmost efficiency, for while it is of little cons
science whether ordinary glasses are placed where their po
increased by 25 per cent., it may make a difference of
decades in her advance if the new artillery opens its attac
heavens from the mountain-tops instead of from the plains."

The same communication also contains the foll

"It is noteworthy that of the 170 new lines found in th
sphere spectrum, not a single one lies below C, and that for
careful examination. The only new lines of much importa
two Hs at the extreme violet end of the spectrum. These
almost constantly reversed, probably quite so, but the obse
so difficult that we could not be perfectly sure of it on every

The following extracts will serve to show
Respighi's observations of the chromospheric line
are from his Fifth Note, published in 1872, in the
Reale Accademia dei Lincei, and read 5th May, 1

"The observations of the last total eclipse of the sun have
the chromosphere, or the gaseous stratum which envelopes
surface of the sun or the photosphere, is not composed only of
but also of many other gases and metallic incandescen
limited however to its base, and reduced to exceedingly t
At the beginning of the totality of the eclipse, when the ext
limb was covered, at the appearance of the chromosphere, n
bright lines of hydrogen were to be seen, but an unlimite
of bright lines, as if there had been a total reversal of the l
solar spectrum: the same phenomenon presented itself at
totality, some moments before the appearance of the ext
limb—a clear proof that, at the base of the chromosphere, th
is mixed with many other substances in the condition
incandescence.

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[1] *Nature*, December 12th, 1872, p. 109.

"In the artificial eclipses produced by our spectroscopes, this complex constitution of the chromosphere is only observed in some almost exceptional cases, and in a very imperfect manner, while in general we cannot obtain anything but the reversal of the lines of hydrogen and of the line D_3, which is held by many as not belonging to that substance.

"This depends on the slight thickness of these gaseous strata, by reason of which they are always veiled and concealed in the undulation of the limb, and thus submerged in a sea of vivid solar light: which does not happen in real eclipses, in which the disc of the moon, covering the sun before the luminous rays can arrive within our atmosphere, renders the undulations, which in full sunlight invade the slit of the spectroscope, impossible.

"But even in full sunlight, favourable conditions are at times presented for observing these bright lines, at least in the case of those substances which, after hydrogen, are most elevated in the photosphere; and this is especially the case in the regions of the spots and of the dark granulations. Indeed, in these localities, I have often been able to observe at the base of the chromosphere, through long tracts of the limb, bright lines besides those of hydrogen, the two red lines between a, B, and B, C, almost always, and very marked. If I have generally confined myself to the observation of these last lines, I was obliged to do so in order not to distract myself by too many operations while I was already sufficiently occupied in observing the solar outline, and in the study of the protuberances. These regions I believe to be those in which Tacchini finds the lines of magnesium reversed. Indeed, comparing the regions in which Tacchini finds these magnesium lines, they appear to me generally coincident with those in which I see the two bright lines between a, B, and between B, C; with this difference only, that these last lines are extended to a smaller part of the outline. But this might depend on the greater difficulty of observing them, because projected upon a bright field, while those of magnesium are more easily rendered visible through the striking contrast of their corresponding black lines. And this supposition is, I think, confirmed by the fact, that the two lines between a, B, and between B, C, are those which, after the lines of hydrogen are most prominent, not only at the base, but also in the most elevated parts of the protuberances; a manifest proof of the great predominance of that substance, from which proceed the lines in question, in the composition of the mass and of the superficial stratum of the sun.

"The visibility of these bright lines in the regions of the spots and of the granulations might be explained by the greater elevation of the gaseous strata, caused by the prevalence of jets or eruptions, generally composed of several substances besides hydrogen; but without excluding that explanation, I maintain that another fact may contribute to render these lines more easily seen.

"It is a fact that with the slit arranged tangentially in the locality of the spots and of the granulations, the spectrum is furrowed or divided by lines almost permanently dark; and this is, I maintain, produced by the dark or less bright streaks of the solar limb underneath the chromosphere, on which the undulations of the outline

NOTE H.

appear less sensible and more depressed, and thence not su conceal the base of the chromosphere; in consequence of wh of the bright lines can then emerge, which lines in the othe the outline are submerged by very active and pronounced un

"In treating, however, of a subject so delicate, I offer my c a simple conjecture, demanding for it the attention and th other observers, in the confidence that the results of their obs will give it a greater degree of probability.

"Professor Lorenzoni has made a particular study of the br of the chromosphere, and in spite of the small dimensior refractor (similar to that of Campidoglio), has, by using s useful contrivances based on the theory of the focal distanc object-glass for the different rays of the solar spectrum, as the almost continuous presence, in the chromosphere, of s refrangible bright lines; such are f. of iron and h. of h from the equator to 75° of latitude, north and south. But as these observations during the period of great perturbation, portant that they should be continued, in order to discover also in the most normal conditions, or in conditions of more solar activity, the visibility of these lines persists through t of the zone indicated; or whether, on the contrary, by the d ance of the large protuberances in the polar regions, this p or characteristic of the chromosphere is limited to the zones o nearer to the equator."[1]

* * * * * *

"The regions of the spots, as I have already shown in my I and III., when they are found on the solar outline, are disting some special characteristics in the chromosphere, as well a protuberances or jets which arise there on the photosphe chromosphere in those regions is generally presented as very and solid, and in conditions such as not only to bring out the hydrogen and D_3 brightly, but also those of various other su among which are sodium, iron, magnesium, &c., and consta red lines, one between B and C and the other between *a* and nearly correspond to the divisions 651·2 and 524·6 of the Kirchhoff.

"The substance which, next to hydrogen, is observed to be r dominant in these jets, is that unknown one, corresponding t lines between *a*B and between BC, already often indicated constantly observed these lines more or less bright at the bases protuberances near to the spots, and I have often been abl and follow them even in the elevated parts of the jets, and at far as 2″, from the limb: occasionally also, by widening the s spectroscope I have observed the complete image of the jets in one as in the other line.

"I have also often been able to observe the lines of m extended to a considerable portion of the protuberances, and 1′ distance from the limb.

"As to the lines of sodium, iron, &c., I have generally able to observe them, except at the base, or in the lowest par protuberances: the lines of sodium only have been sometimes observed at some distance from the limb.

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[1] Respighi, Note v. § I.

"As regards those jets which are compound, their general and fundamental *trait* is that they contain hydrogen from which the other substances seem to be carried off to a greater or less height, only in some especial ramifications; so that the hydrogen must be considered as the fundamental and predominant element of all the solar eruptions. In the very numerous and very accurate observations made by me of the eruptions near to the spots, I have been unable to observe jets of any substance not accompanied by hydrogen.

"In my Note III., in treating of the protuberances with complex spectra, I remarked that the two red lines between *a*B and between BC often appeared very vivid in some parts, and especially in the summit of some jets, while they were totally wanting in the lower portions, so that the gas corresponding to these lines constituted true clouds enveloped in hydrogen. This fact I had previously verified also for magnesium, of which I had often seen clouds mixed with hydrogen to a remarkable height from the solar limb, almost as high as 1′.

"This fact seems to me coincident with that of the nodes or nuclei found by Professor Lorenzoni in his exceedingly accurate observations; he observed these often in the protuberances with complex spectra; distinguished from the other parts of the protuberances themselves by the multiplicity of the spectral lines, namely, *f*, *b*, D, &c. Generally it is ascertained that the visibility of these lines in the elevated parts of the jets is of brief duration; which would prove that the substances to which they belong constitute jets or intermitting eruptions of short duration, returning rapidly to the surface of the sun, or rapidly losing the vividness of their brilliancy.

"In the jets near to the spots, or rather, in those points or bright solid masses which are often observed in proximity, and sometimes in contact with the nuclei, the spectrum is generally composed of very numerous bright lines, of which I have often very distinctly seen as many as twenty, although I was not expressly engaged at the time in the observation of them. On some rare occasions, the whole spectrum appeared momentarily reversed or luminous beneath these masses of bright points, and sometimes I remarked a fact, which seems to me worthy of notice; namely, that the spectrum became luminous in some portions only, which beginning at the lines C, D_3, and F, extended to some distance towards the least refrangible parts of the spectrum, gradually diminishing in brilliancy."[1]

Father Tacchini has observed the following bright lines in the spectrum of a prominence:—Hydrogen, D_3, B, C — B, *a*, b_1, b_2, b_3, b_4, 4943 (double), 5031 (double), 5194, 5229, 5272, 5282, 5265, 5316, and two other undetermined lines between *b* and 5316.[2]

Professor H. C. Vogel has observed the following bright

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[1] Respighi, Note v. § ix.

[2] *Comptes Rendus*, vol. lxxvi. p. 829, March 31, 1873.

NOTE H.

lines. He gives the heights to which they extend the solar limb :—

	Wave-length.	Height above limb.	Substan
C	656·2	10"	H
D_3	587·4	10"	?
	531·7	6	Fe
b_1	518·3	6	M
b_2	517·2	6	M
b_3	516·8	5	Ni
b_4	516·7	4	M
	501·3	4	Fe
	492·3	6	Fe
F	486·1	8	H
	434·0	6	H

Later he made the following observations :—

	Wave-length.	Height above limb.	Substance.	Coincidence w sponding dark
C	656·18	20"	H	Perfect
D_1	589·50	8	Na	Perfec
D_2	588·90	8		
D_3	587·43	20"	?	
	531·55	3	Fe ?	Not perfect line has length 53
b_1	518·30	10	Mg	Perfec
b_2	517·20	8	Mg	Perfec
b_3	516·83	5	Ni	Perfec
	501·76	3	Fe	Coincides cisely with line of th
	492·31	3	Fe	Perfec
F	486·06	20	H	Very wide the centr exactly.

The magnesium line b_4 and the third hydro were not seen.[2]

Professor Lorenzoni[3] gives the following stateme frequency of appearance of some of the lines seen Of twenty-six protuberances seen in one month (

C D_3 F Hγ	were seen in	26
f	"	19
h	"	17
The four *b*'s	"	8
The two D's	"	6
The three lines, one of which is between B and C and the other two between *b* and F		1

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[1] This is the 1474 line of the corona.

[2] H. C. Vogel, *Beobachtungen*, 1872, pp. 36-37.

[3] *Memorie della Societa degli spettroscopisti Italiani*, vol.

NOTE I.[1]

THE LINE 1474 (KIRCHHOFF).

NOTE I.

This line was first observed by me in the chromosphere on June 6, 1869 (p. 496). It was then seen by the American astronomers in the corona in the eclipse of that year. My most recent work with regard to it, showing that it cannot be an Iron line, will be found referred to at p. 553.

Professor Young's and my own early opinions as to the origin of this line will be seen by reference to pp. 268 and 271.

Secchi, in 1869, suggested that an investigation of the *hydrogen* spectrum would throw light on the subject.

He says:—

> "L'hydrogène nous a donné, dans la boule du tube du pôle positif, une raie assez belle, placée presque à égale distance des deux raies C et F (α et β) du spectre solaire, mais un peu plus rapprochée de F : or cette raie coinciderait avec celle que M. Young a observée dans la dernière éclipse, comme propre à la couronne. On l'a attribuée au fer, et j'ai d'abord supposé que, dans mes tubes, elle pouvait provenir des réophores métalliques ; mais les tableaux que je possède ne m'autorisent pas à admettre cette conclusion ; je l'ai vue d'ailleurs également dans des tubes de Geissler, qui sont très-purs et très-peu fluorescents ; je crois donc qu'elle appartient réellement aussi à l'hydrogène, mais qu'elle se développe sous une température plus basse, car je l'ai vue seulement dans la boule qui environne le réophore."[2]

Professor Young has been independently led to consider this line as probably not due to iron, from the fact of its want of sympathy with other undoubted iron lines:—

> "The corresponding line in the spectrum of iron is feeble, and on several occasions, when the neighbouring lines of iron 1463 have been greatly disturbed, this has wholly failed to sympathize ; hence I have marked the Fe with a ?"[3]

In the same year, Professor H. C. Vogel remarks, when dealing with the chromospheric lines, "5315·5 eisen? Schien nicht genau zu coincidiren, die eisenlinie hat die Wellenlänge 5315·9;"[4] thus indicating a difference of

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[1] See p. 496.

[2] *Comptes Rendus*, vol. lxx. p. 84, January 10th, 1870.

[3] *American Jonrnal of Sciences and Arts*, vol. iv., November 1872.

[4] H. C. Vogel, *Beobachtungen*, &c., 1872.

NOTE I.

0·4 mm. on Ångström's scale, bet
the line ascribed to iron on Ångs
coronal line with which it is generall

NOTE K.[2]

The Continuous Spectrum A
Chromospher

NOTE K.

Soon after I had first announced
prominences by means of observati
an eclipse, a further examination of
sun enabled me to detect the brigh
of its contour.

In the same year that the disco
sent a note to the Paris Academy, i
following announcement. The lett
November, 1868, and is printed in t
the 25th of that month :—

> "Mais ce qu'on n'a pas encore remarqué,
> voisinage du bord solaire, même là où la raie
> la raie noire disparaît et le spectre devient
> un effet de contraste, mais une dispositio
> renversement est seulement partiel. La m
> F et dans plusieurs autres raies."

On the 1st March, 1869, he addres
to the Academy of Sciences at Pa
the following words :—

> "Ayant agrandi notablement, au moyen
> Soleil qui tombe sur la fente du spectros
> aperçu que, en y faisant entrer lentement le b
> les protubérances, et la couche rose ; pui
> évanouies et que les lignes brillantes ce sont
> alors, on voit apparaître le bord solaire. J'
> tion, que, en général, *la couche rose est s*
> une distance minime mais appréciable."

A little further on, in the same
mettant la fente tangente au bord, o

---

[1] Recent observations have led me to th
made in Ångström's map, and that there is

[2] See p. 497.

[3] *Comptes Rendu*

rose n'est pas en contact continu avec le bord solaire." He states that this is only seen in perfection with a magnified image.

He then proceeds to examine his observations in the following way:—

> "A ma grande surprise, j'ai vu alors disparaître complètement toutes les raies fines, et les raies noires D et *b* restait à peine visibles. J'ai d'abord cru que c'était là un effet de la faiblesse de la lumière, mais j'ai vérifié qu'il n'en etait pas ainsi, car je pouvais voir très-bien les raies au dehors de la couche rose, et immédiatement sur le bord du Soleil. J'ai attribué l'effet observé à la faiblesse du spectroscope, et je l'ai avait monté avec trois prismes très-dispersifs: le résultat a été le même. On pourrait supposer que le phénomène est dû à un effet de l'agitation de l'air, près du bord solaire, mais cette hypothèse elle-même m'a paru devoir être exclue par cette remarque que je distinguais très-bien les raies brillantes de la couche rose au milieu de l'oscillation la plus vive: les raies fines obscures sont alors visibles dans tous leurs détails. Il me paraît donc que nous sommes en face d'une couche qui donne réellement un spectre continu."[1]

He then adds that this stratum is difficult to observe, on account of its extreme thinness.

Further on still, he says—"*Cette couche, proportionellement assez mince, serait celle où a lieu le renversement selon la théorie de Kirchhoff;*" and then proceeds to argue, that though very thin, it is deep enough for the purpose.

References to p. 497 will give my views of this matter, which are simply, first, that I have never seen an absolutely continuous spectrum in the place indicated by Secchi, though I have often seen nearly a continuous spectrum given by faculæ, an observation endorsed by Captain Herschel.[2] Secondly, that a considerable reduction in the number of absorption lines observed here is easily accounted for by the fact that two spectra are superposed; and thirdly, that I cannot admit that "*la couche rosc n'est pas en contact continu avec le bord solaire.*"

When Professor Young in the clear atmosphere of Mount Sherman saw many lines reversed, he considered it might be the stratum to which Secchi referred, but on

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[1] *Comptes Rendus*, vol. cit., p. 581.

[2] *Proc. R.S.*, vol. xviii. p. 64.

NOTE K. this I remark that, although Secchi has claimed th ment in support of his position, nothing like a co spectrum was observed by Young, and nothing about the said stratum being separated from t limb.

Hence I hold that, although both kinds of obse —eclipse and non-eclipse observations—concur i ing that at the base of the chromosphere the nu vapours present is greater than was at first supp are still as far off as ever from a stratum givi continuous spectrum *lying between the hydrog the sun.*

I here give the extract from Professor Young's to which I have alluded. He says:—

> "At the very base of the chromosphere, and to a distance o 1″ or 15″ from the edge of the photosphere, it was found t dark lines which are not actually reversed lose their inten vanish more or less completely. This is substantially a con of an old and somewhat disputed observation of Secchi's, wh at the edge of the sun a layer giving a continuous spectrum.
>
> "This is not strictly correct, however, since when the tran of the air is so much increased as to cause the most persiste dark lines to vanish, a multitude of the others appear reversed can be little doubt that were the effect of our own atmospher removed, this lowest portion of the solar atmosphere would same spectrum of bright lines which is seen at the begin end of totality during an eclipse."[1]

NOTE L.[2]

THE CLASSIFICATION OF PROMINENCES.

NOTE L. As early as 1869, I had in my mind divided prom into two great groups (see p. 484), but it was April 27, 1870, that I communicated the classific them into Eruptive and Nebulous to the Royal (see p. 518).

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[1] "The Sherman Astronomical Expedition," *Nature*, I 12th, 1872, p. 108.

[2] See p. 518.

In the sitting of the Saxon Academy, on the 2nd of June, in the same year, Zöllner, in a paper laid before that body, announced that he had arrived at the same result:—

"The forms of the protuberances may be divided into two characteristic groups—into the vapour or cloud-like, and into the eruptive formations. The preponderance of the one or the other type appears partly to be dependent on local conditions on the surface of the sun, partly on the time; so that at particular periods the one, at others the other type preponderates. The striking resemblance of the cloud-like formations to terrestrial clouds is readily explained, when it is considered that the forms of our clouds are due not to the particles of water suspended in them, but essentially to the nature and matter in which the differently heated and agitated masses of air are spread out. *The particles of aqueous vapour are, in terrestrial clouds, simply the material by means of which the above-mentioned differences between the masses of air are rendered evident to us. The glow of the incandescent masses of hydrogen is the cause of the visibility of the clouds of the protuberances.*"

Young at once accepted this classification:—

"About forty different prominences have been more or less carefully observed from September 10 to October 3, 1870; sixteen have been sketched. Most of them fall naturally enough into the categories established by Zöllner and Lockyer."[1]

On the 4th December, 1870, Respighi read before the *Reale Accademia dei Lincei* his third note, in which was contained the very careful and elaborate classification given below:—

"In the immense variety of forms of the protuberances, the following types are chiefly to be remarked:—

"I. Well-defined, delicate, and isolated jets.

"II. Jets united in groups.

"III. Jets with ramifications and dispersions.

"IV. Jets of larger section, columns or pyramids, cloudy and isolated.

"V. Jets or cloudy columns united in groups, intermingled at the summit with cloudy arches.

"VI. Cloudy masses, irregular and supported on the solar disc.

"VII. Masses or clouds separated from the solar circumference.

"The delicate and well-defined jets are observed oftener in the locality of the spots, where generally they are seen to rise above the chromosphere in very light threads, more or less long, more or less inclined towards the vertical: at times rectilinear, and clouded at the summit; at times curved inwards in parabolic form on the disc of the sun.

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[1] *Journal of the Franklin Institute*, 1870; *Nature*, vol. iii. p. 112.

NOTE I.

"The jets united in groups often present in the locality spots some very light threads, near to one another, now paral diverging. Also in the other parts of the solar surface, n large groups of jets, very defined and delicate, are to be obser jets rather less light, and intermingled irregularly in such a that if the air is not very transparent and tranquil, they appe like cloudy masses, terminating at the summits in more or less filaments.

"This last form of protuberance is more frequent in high la and we often see some of them rather large, even to 70° of latit

"Of this classification of the forms of the protuberances, it is that the fundamental forms or the constitutions of the protub themselves are two; and these are, that of the cloudy mass that of well-defined, delicate, and isolated jets, or jets un groups."

It will be seen, then, that Professor Respighi a my fundamental classification.

I also had observed that the cloudy prominences of them extending over an arc of 30° or 40°—chiefly a the polar regions. In this I am also borne out by Pro Respighi, who states:—

"These gigantic clusters of protuberances are generally preser environs of the regions of the spots, and more frequently in the of them near the poles. Although this suggests continual var nevertheless these protuberances present a greater persisten duration than those which arise in the locality of the spots, wh quickly developed and dispersed, as I have already remarke previous Notes. Sometimes large groups of protuberances o the higher latitudes also, even up to the poles, but they ar fainter in brightness and less solid in structure.

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[1] Note iii. § 2, p. 9.

[2] Note v. Atti, p. 263. He then goes on to add:—

"Lately some observers have mentioned these two fundament tures, offering this distinction as a novelty, as a most importan brought to light by their observations made with large instr and pronouncing the small instruments to be insufficient for the ing of the fibrous structure or of delicate jets, and asserting observer had before succeeded in observing them. I do not necessary to waste any words in showing that this assertion is unfounded; this having been clearly proved by my former ann ments above quoted: since by them it is not only evident tha already long ago fully characterized this form of protuberan more than this: I had indicated the conditions and the locality solar surface in which they were predominant. In the solar appended to Memorandum III., in spite of the smallness of t and the imperfection of the drawing, this structure of protube distinctly indicated, especially in the regions near the poles."

"Generally, these consist of jets or fine threads, either parallel, converging, diverging, or curved at the summit, and often converging to a conical or pyramidal form. These jets or threads are occasionally cut short at the base, presenting the appearance of a shower of fire; and in spite of the weakness or the slight intensity of their light, they are shown to be rather more consistent and enduring than the vaporous masses. Generally the large protuberances can be divided at their base into many distinct jets of various sizes and degrees of brightness, which at their summits are diffused in the most curious way, being blended in a mass of light, often terminated irregularly in feathers or ramifications either rectilinear or curved.

"Sometimes, however, we see in the vicinity of the spots, masses of hydrogen, solid at the base and cloudy at the summit, of moderate height and extent, and extending over several degrees of the solar limb.

"Also, in the isolated masses or clouds the dimensions are exceedingly variable, some of them being reduced to delicate threads, isolated or combined in groups; others, on the contrary, are vaporous masses, more or less solid, and more or less irregularly terminated, and of enormous volume."[1]

In October 1871,[2] Secchi sent to the Paris Academy a classification of his own. Of this I give an abstract; it will be seen that it agrees exactly with the divisions of former observers, to whom, however, he makes no reference whatever.

He mentions the following classes:—*Amas, jets, panaches.*

The first he subdivides into *amas brillants* and *amas cumuliformes*. These two sub-classes include all the dense and massive prominences; the first sub-class is considered as consisting of local upheavals of the chromosphere not higher than 15″ or 20″.

The second class, *jets*, includes all the narrow and pointed prominences. The meaning of the term is so well understood, that no further description is necessary.

The third class, *panaches*. includes all not comprehended in the two former classes, such as all the jet-like prominences of considerable width; the smoke-like and tuft-like masses; those like those; the interlacing filamentous; and those which present that curious cellular structure so rarely seen are mentioned as included in this group. In a

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[1] Note v. Atti, p. 268.

[2] *Comptes Rendus*, lxxiii. pp. 826-836, October 2, 1871.

NOTE L.

separate group altogether from pr
clouds.

He then observes :—

> "En distinguant entre les *jets* et les *p*
> l'intention de préjuger la question de savo
> pas aussi des jets : cette question n'est pas
> La distinction réelle nous paraît consister
> une partie de la photosphère semble soul
> panaches, la seule partie affectée est la chr
> jamais trouvé dans leur spectre que les lig
> raie D_3. Mais il nous paraît pas qu'on pui
> tous ces jets offrent un orifice d'émission da
> sort le gaz, et bien moins encore qu'on pui
> protubérances comme mesure de la pression
> voyons les panaches se produire même da
> dans l'atmosphère libre loin de la chromo
> admettre une telle couche liquide."

Spörer has accepted my classific
returned to it.

NOTE M.[1]

CONNECTION BETWEEN PROMIN
FACULÆ.

I. *Spots—Facul*

NOTE M.

On this head I need say nothing
scarcely have spots without faculæ,
spots are common.

II. *Spots—Promin*

The first suggestion on this sub
a telegram from M. Janssen, dated 12
Simla.[2]

"*Dépendance entre la présence des tac*

Father Secchi at once accepted thi

> "Je viens de recevoir les *Comptes Ren*
> trouve une communication très-intéressant

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[1] See p. 520.

[2] *Comptes Rendus*, vol. lxv

relation entre les protubérances et les taches. De mon côté, j'étais déjà arrivé à la même conclusion. Les 4, 5, et 6 Janvier, j'avais remarqué que, près des taches, la raie noire C disparaissait, ce qui prouve que la lumière de l'hydrogène était assez forte pour compenser l'absorption du reste de l'atmosphère solaire. Dans l'intérieur des taches, on ne voit pas de raie brillante. C'est surtout dans la région des facules environnant la tache que la raie C s'affaiblit, ou disparaît complètement. Il paraît qu'on ne pourra jamais voir directement les raies brillantes sur le soleil lui-même ; mais la disparition de la raie C suffit pour constater la présence d'une protubérance."[1]

I could not accept this conclusion (see p. 493) in all its generality, and was driven to associate the prominences more with faculæ than with spots, the prominences seen near spots being due to the faculæ with which the spots are always accompanied (see p. 521), the more as the prominences near spots are composed of jets at some period of their life.

Young is also of this opinion :—[2]

"From the observations above detailed, it is evident that the spots and prominences obey nearly the same law in respect to their distribution on the solar surface ; but the prominences, which are far more numerous than the spots, approach nearer to the poles and are more frequently found on the equator.

"I have never yet been able to watch a spot in its passage around the limb so that I could observe its effects on the chromosphere, but my present impression is that certain depressions from time to time observed in the chromosphere (see Fig. 19), are due to spots directly under them. In only one case (No. 5. Nov. 4th) have I found a prominence very near a spot, and then only a small one. The spot referred to in connection with No. 9, Nov. 4th, was about 25″ from the limb. Neither did spots make their appearance at or near the base of the great protuberances observed Oct. 7th. If they had they must have been seen on Oct. 11th. Whether the prominences are connected with the faculæ is a different question, and more likely, I think, to receive an affirmative answer."

Still, though it was not true that all prominences were associated with spots, I pointed out, in April 1870, that the prominences which indicated the spectrum of the lower vapours were *generally* associated with spots, including of course the facula platform (p. 518), while the cloud-like prominences *generally* did not. My statement

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[1] *Comptes Rendus*, vol. lxviii. p. 237, February 1869.

[2] *Journal of the Franklin Institute*, No. 528, p. 423.

NOTE M.

of this connection has since been entirely endors
Respighi and Secchi.

The following are some of the most important c
fessor Respighi's observations on this point :—

> "Although the locality of the spots may usually be distin
> from others by an energetic state of eruption, yet occasionall
> seems to reign a condition of calm, with the single peculiar
> remarkable intensity and regularity in the red stratum, either f
> total absence of protuberances or from the presence of few a
> small jets.
>
> "From many observations made by me on the solar limb,
> locality of the spots, the following results are deduced :
>
> "1. Red stratum is very low, very regular, and very bright.
>
> "2. On the nucleus, the red stratum is rather low and
> totally absent.
>
> "3. On the nucleus, either eruptions do not take place at
> they are confined to fine jets which last but for a short time.
>
> "4. On the parts around the spots gaseous jets arise, which
> remarkable violence and intensity, being moreover very def
> form.
>
> "5. The jets near to the spots are not constituted of hydroge
> but of other substances also, as manifested by the respective
> bright lines.
>
> "6. Among these bright lines, which are generally met wit
> base, or in the lower portions of the jets, those of sodium, mag
> and iron are often to be observed, and constantly the two lines
> red, one between B and C, the other between B and *a*, which a
> extended by notable streaks of the red stratum, and occasion
> shown very distinctly even on the elevated parts of the jets.
>
> "7. From time to time in the neighbourhood of the sp
> eruptions assume temporarily gigantic proportions ; and by th
> probably produced those rapid transformations and those sud
> placements which are sometimes to be observed in the spot
> selves.
>
> "8. In the locality of the spots, we often meet with jets cu
> wards on the solar disc, in shapes plainly parabolic, at times i
> at times united in groups.
>
> "9. The large jets or masses cast out in the vicinity of the s
> developed and disappear more rapidly than in other localitie
> quickly return to the surface of the sun."[1]

It will be perceived that Professor Respighi allu
the absence of the chromosphere on certain occ
over spots when observed near the limb. This is a
of great importance, as it shows that my origina
that the connection of the jets is really with the

---

[1] Respighi, Note v. § ix.

surrounding a spot, and not with the nuclæi, is most probably correct. I have myself observed this, and can confirm his observations.[1] The statement, however, was violently attacked, which brought out the following reply from Respighi :—

"In bringing forward these characteristics of the chromosphere and of the solar eruptions in the locality of the spots as results of a period of observation, it was certainly not my intention to establish unvarying laws and data, since I admitted that there are periodical variations in the solar eruptions, which would render it possible that these conditions also might be subject to great modifications and exceptions. Although I admitted this, I have been unjustly taxed with inaccuracy in my deductions from observations. If during other periods of observation some of these results failed, this ought to have been attributed, not to inaccuracy in my observations, but to the difference in the conditions prevailing in the two periods, differences in the surface and in the solar body. But this is not the spirit in which some astronomers work, some who, offering the greater part of these results as novelties originating from their own observations, have not scrupled to denounce as inaccurate those few results which did not appear to agree with these observations of theirs, made during a totally different period, and during a state of strong perturbation in the solar activity.

"Thus they declared to be inaccurate the result of the depression and regularity of the red stratum in the locality of the spots, and of the temporary conditions of calm in the regions themselves, because during those observations the chromosphere appeared irregular, being covered with small jets and abounding with frequent eruptions. But this fact, instead of contradicting the results of my previous observations, merely shows that during the period of great perturbation (in the locality of the spots, as well as throughout the whole of the solar surface) the solar activity is more energetic, and thence arise the irregularities of the chromosphere and the greater frequency of eruptions. This fact also gives evidence of the great influence which the eruptions have on the production, transformation, and displacement of the spots; since during this period the appearance and disappearance of the spots were very frequent and the transformations continuous.

"But in spite of the extraordinary activity in this period, not a few also cases occur of spots being on the limb or near to it, without being accompanied by notable eruptions, and only with the chromosphere bright, low, and with some small jets. Among the results of my early observations, that of the great depression of the red stratum on the nucleus of the spots has been pronounced inaccurate : from which depression I was led to suspect that perhaps on the nucleus the hydrogen does not exist in the luminous condition.

"The argument brought against this conclusion of mine is founded on the fact that the red stratum in or over the nucleus of the spot is often sensibly elevated.

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[1] See also Young, in *Journal of Franklin Institute*, No. 524, p. 142, Aug. 1860, and quotation on page 629.

NOTE M.

"The argument would be conclusive if it had been proved risings of the chromosphere really occur on the nucleus ; the other hand, they may appear on it only by projection ; a very easy to verify by the eruptions or protuberances which abound on the surroundings of the nucleus itself.

"To decide this question, we must select the most favoura tions, that is we must follow the nuclei until they disappe solar outline, when the chromosphere is not sensibly alter projection of the protuberances to be found in the regions su the nucleus.

"It is precisely in these circumstances that I was very oft 1870 to observe that extraordinary attenuation of the red str occasionally also breaks and indentations in it, in the preci of the nucleus : and in previous observations, notwithsta great frequency of the eruptions and of the little risings c stratum, I was able to observe the same fact in the spots w visible at the limb with the chromosphere calm, or in some f interval of quiet between the eruptions.

"Observing the spectrum of large spots near to the limb slit of the spectroscope tangential to it, and following them projection on the limb itself, I have been occasionally able t the real interruption produced by the spots on the solar lin the limits of the photosphere, by observing that by the int into the slit the summit of the undulations of the outline, th formed by their spectra was separated by a band or line per dark, coinciding with the locality of the nucleus : and I h ascertained that, at the disappearance of these rays, the sp the locality of the spots remained dark until after the disap of the chromosphere. But these observations are very deli require exceedingly favourable conditions, which cannot be out observing the large spots assiduously and patiently on thei ance or disappearance at the limb."[1]

The fact, however, has been again absolutely by Father Secchi, in a paper communicated to th Academy this year :—

"Je dirais qu'après des centaines d'observations, je n'a rencontré la chromosphère absente, ni sur les taches, ni ailleu une fois, où cette enveloppe était absente dans l'étendue d degrés héliocentriques, mais cela très-loin de toutes les tac suis très, disposé à croire que cela tenait à l'intervention de corps inconnu, entre nous et le soleil. Cette observation fu 28 Octobre, 1871, à 107 degrés de position du pôle solaire l'est."[2]

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[1] Respighi, Note v. § ix. pp. 55-60.

[2] *Comptes Rendus*, vol. lxxvi. p. 1053, 28th April, 18

III. *Prominences—Faculæ.*

From my observations, communicated to the Royal Society in 1870, it will be seen that I am inclined to associate the prominences with the brighter parts of the faculæ; I do not hold to an absolute connection; but I do hold to the lozenge as being the birth-place of prominences, although in the fainter prominences the pressure at work is too small, doubtless, to give rise to this appearance. NOTE M.

Professor Respighi has stated his opinion on these points as follows. It will be seen that on one point we are not in agreement:—

"Generally in the locality of the faculæ the protuberances or the eruptions are more frequent and more developed, and it seems possible to conclude that the faculæ are never unaccompanied by protuberances, while, however, there may be protuberances without faculæ.

"Although near the faculæ are commonly found large protuberances, yet their positions are not proved to be so coincident as to allow us to consider them inseparable. The protuberances and the jets lie near the faculæ, but constitute a phenomenon totally distinct from them.

"On the other hand, this does not disprove that between these two phenomena the closest relations may exist; nor does this exclude the idea that one of them may be necessarily dependent upon the other. I have duly considered the concomitance of the faculæ with the protuberances and with the eruptions, and it seems reasonable to suppose that either from the faculæ the protuberance is produced, or *vice versa.*

"Taking into consideration, however, that large protuberances are also seen near to the poles, in which regions there are no faculæ, it seems to me more reasonable to suppose that the faculæ may be a consequence of the protuberances, or rather of the solar eruptions; from which might be produced in the photosphere condensations, local accumulations of that bright or inflamed substance which constitutes those points, or lucid grains with which the solar superficies is covered; and this seems to me confirmed by the fact that those flames are so much the more vivid, the more violent and gigantic the jets by which they are accompanied. Nor to this can we oppose the fact (at times verified) of faculæ not accompanied by noticeable protuberances, since that may be explained by admitting that the faculæ may be persistent, or remain visible after the cessation of the eruption.

"The presence of protuberances without faculæ (a phenomenon often verified, especially in the great distances from the equator) probably depends on this,—that to produce the faculæ the eruption is not sufficient, but it is required that there should be special conditions, perhaps in the eruption, perhaps in the constitution of the photosphere in that place where the eruption is developing.

NOTE M.

"In the spot zones probably the faculæ are more defined
frequent, because the eruptions are more frequent, more gig
more violent, also the constitution of the photosphere is m
or heterogeneous."[1]

Father Secchi, who had at first arrived at the co
before referred to, now makes the following sta
the italics are Secchi's own :—

"Quant à la connexion entre les protubérances et les facul
établir ce qui suit : *Les facules accompagnent invariablemen*
quelque soit leur forme, mais les panaches en sont bien so
dépendants, surtout s'ils sont petits."[2]

Again :—

"Sous la dénomination de *jets*, je comprends ces flam
et brillantes qu'on trouve surtout dans le voisinage des tach
la couronne brillante de facules qui les environne."[3]

"Il est remarquable que, toutes les fois que l'on obser
d'une grande vivacité au bord, on y voit aussi un facule.
suis convaincu en observant l'image solaire projêtée et agra
le dôme noire de l'équatorial de Cauchoix. Sans doute o
pas attendre à la réciproque, c'est-à-dire à voir toujours u
où l'on trouve une simple proéminence nuageuse, car la lu
moins intense, et les facules faibles disparaissent au bord.
marquant les régions faculeuses de l'image solaire, et
qu'elles arrivent au bord, on trouve que là il y a toujours
grande quantité de protubérances, ou au moins une chro
plus élevée.[4]

Secchi gives a later description of the behaviour
spots and faculæ, and arrives at the following conclu

"1° Les indices d'éruption des taches, constitués par le
ment des raies de l'hydrogène et par les dilatations des raies
vapeurs métalliques, sont des indices rationnels et certains
tence réelle de ces éruptions. Ces modifications des raies
l'équivalent du renversement qu'on observe au bord." (Se
and C.)

"2° Les taches passent par deux périodes bien distinctes
formation et celle de dissolution ; la présence d'une tache a
permet pas de conclure à l'existence nécessaire d'une érupti
car la tache pourrait bien être dans sa deuxième phase de di
. . . . On prouve encore ainsi que la tache est l'effet des ér
qu'elle en dérive comme je l'ai exposé ailleurs.

"3° Ainsi se trouve encore confirmé ce que nous avons con
de fois, que les facules très-vives, surtout en présence des ta

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[1] Respighi, Note v. § vii.

[2] *Comptes Rendus*, vol. lxxiii. p. 836, Oct., 1871.

[3] Op. cit., vol. cit., p. 828. October 1871.

[4] Op. cit., vol. lxxii. p. 355, 20th March, 1871.

accompagnées par des éruptions, et qu'elles déterminent une élévation assez sensible sur le bord solaire. Sans doute la facule n'est pas la protubérance, mais comme sur ces facules il y a toujours ou éruption ou vivacité extraordinaire, avec soulèvement de la photosphère, comme l'a prouvé M. Tacchini, et renversement des raies métalliques, une élévation visible de la chromosphère elle-même ne peut plus être contestée."[1]

NOTE M.

This precisely accords with the views I put forward in July 1869, so far as the connection in question goes.

NOTE N.[2]

THE EXPANSION OF LINES AT THE BASE OF THE CHROMOSPHERE.

NOTE N.

With regard to the widening of F, Father Secchi at first admitted it, and accepted the explanation given:—

"J'ai encore porté mon attention sur la largeur des raies brillantes de la chromosphère, et j'ai constaté que, en général, les raies principales sont toutes trois plus larges à la base qu'au sommet, ce qui prouve l'influence de la pression exercée par les couches supérieures sur les couches inférieures, comme il est naturel de l'admettre."[3]

He then began to doubt the fact, and endeavoured to show that with a large image of the sun it is not observed:—

"En effet, en employant l'image directe, j'ai vu la raie F très-brillante et en forme de fer de lance, et en ouvrant un peu la fente on voyait toute la protubérance et on relevait son contour ordinairement conique. Mais ces apparences s'évanouissent en grossissant l'image solaire, car la protubérance acquiert alors un hauteur linéaire plus grande, et une largeur qui surpasse plusieurs fois la largeur de la fente.

"M. Lockyer insiste beaucoup sur l'élargissement de cette raie F à la base, mais j'ai des doutes sur l'importance qu'il lui attribue. En effet les lignes lumineuses F, C, ou autres que nous voyons, ne sont pas des objets réels : elles sont seulement l'image de la fente éclairée par la lumière d'une certaine réfrangibilité. Cette image doit, pour une lumière homogène, être terminée par deux lignes parallèles, lorsque l'image de la protubérance a une largeur supérieure à la largeur de la fente. C'est réellement ce que je vois habituellement, car la raie F se présente avec une longueur apparente de 1 à 2 centimètres[4] et elle traverse parfois toute la largeur du spectre. Cette ligne

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[1] *Comptes Rendus*, vol. lxxii. p. 320 (1871).

[2] See p. 526.

[3] Op cit., vol. lxix. p. 41, 5th July, 1869.

[4] Cette manière d'énoncer la grandeur apparente est sans doute vulgaire, mais elle est assez commode pour donner une idée de a grandeur du phénomène observé.

NOTE N.

est toujours terminée par une pointe effilée, comme les aut
et D_3. S'il y a donc une dilatation à sa base, elle peut
deux causes : 1° d'une irradiation, due à la vivacité de la l
y produit une expansion apparente ; 2°, d'une différence d
bilité, qui, ajoutant de nouveaux rayons à droite et à ga
raie, l'élargit aussi. Ces deux causes sont également p
probables, et il reste à trouver la véritable.

" C'est d'abord un fait bien constaté que l'intensité de la l
protubérances n'est pas toujours la même, et que l'irra
parfois assez forte pour donner à la raie C la forme de coin
plus dilatée là où elle est plus vive ; mais, en mettant soig
au point focal de vision la fente, et surtout en amplifiant l'i
protubérance, comme je le fais habituellement, on trouve to
portion rectiligne terminée par une pointe. Et s'il y a
suspendu, on voit nettement le milieu du nuage rectiligne
trémités en pointe effilée. Cette pointe, du reste, peut bien
soit par la densité du nuage, soit par l'intensité de la lumièr

He then described some experiments on hyd
tubes, and, admitting that the lines of the gas a
certain conditions enlarged, attributes this enlarg
the effects of temperature :—

" Si dans l'hydrogène, on affaiblit graduellement la t
l'étincelle, on remarque que la largeur des raies $H\alpha$, $H\beta$, H
aussi, jusqu'à ce que, dans les tubes à grand section, elles
sent ou deviennent très-fines et à peine visibles avec la mê
ture de fente. Il paraît donc qu'il y a une température li
une densité donnée, à laquelle les raies brillantes α, β, γ,
s'évanouissent. Mais quelle est cette température ? C'est
très-difficile de déterminer. Je dirai seulement qu'elle doit
élevée, car les tubes capillaires, quand le courant a pass
quelques instants, s'échauffent tellement, malgré la grande
du verre, qu'on ne peut pas les toucher. . . . . . On voit q
tion de température donne à l'hydrogène la propriété de
dans son spectre, des raies plus larges. Or les protubér
sentent leurs raies spectrales élargies à la base, près du disq
au sommet, les raies sont terminées en pointe, là où la temp
plus faible ; . . . .

and considers that the lines in question may by
haviour enable us to estimate the solar temperatur

" De ce que nous venons de dire, il paraît résulter que l
nation de la température solaire pourra être effectuée, si l'o
fixer quelle est la température à laquelle, sous une certaine p
gaz cesse de donner des raies brillantes."[2]

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[1] *Comptes Rendus*, vol. lxix. pp. 316, 317, 2nd August,

[2] Op. cit., vol. lxx. pp. 83, 84, 10th Jan., 1870.

NOTE N.

At length in March 1871, he admits the fact of the widening of the F line in the solar spectrum :—

> "Dans cette protubérance j'ai pu vérifier aussi la structure de la raie F, en forme de fer de lance, que je n'avais jamais rencontrée, et qui a été vu par M. Lockyer. J'ai vu aussi la dilatation de la raie C en forme de losange telle que la décrit cet auteur, et les distortions des lignes D_3 et F, que je désirais vivement vérifier."[1]

M. Rayet admitted the widening of the line F, and also described that of the solar line *h* (Hδ) as early as the middle of 1869 :—

> "L'examen de l'atmosphère solaire m'a montré . . . . . une ligne brillante située entre G et H de Frauenhofer. . . . . . Cette raie brillante, souvent globuleuse comme F, ne se voit d'une manière très-nette que dans des circonstances favorables. . . . ."[2]

Young, before the eclipse of 1869, had seen the broadening at the base, and also saw it (I saw the broadening of 1474 in 1871) during the eclipse (p. 253) :—

> "The F line in the spectrum of the great protuberance was absolutely glorious, broad at the base and tapering upwards, crookedly, as Lockyer has before often described."

NOTE O.[4]

WELLING-UP OF MAGNESIUM AND IRON.

NOTE O.

The first observation of this phenomenon was that recorded in my fifth paper (see page 354), and since then the reversal of the magnesium lines has been repeatedly observed as an ordinary occurrence in the chromosphere.

Tacchini has observed this upon two occasions, first on the 6th May, 1872,[5] when it extended over arcs of from 12° to 168° in extent ; secondly, on the 18th June of the same year, when the whole limb exhibited the reversal of the *b* lines as I had previously observed.[6] On this oc-

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[1] *Comptes Rendus*, vol. lxxii. p. 363, 27th March, 1871.

[2] Op. cit., vol. lxviii. p. 1321, 1869.

[3] Professor Young's Report of the American eclipse of 1869.

[4] See p. 495.

[5] *Comptes Rendus*, vol. lxxv. p. 23, July 1, 1872.

[6] Op. cit., vol. cit., pp. 24 and 25.

NOTE O.

casion he states that a difference in the light of
was distinctly visible to the naked eye. He st
that "le nombre des petites facules était par
d'accord avec la présence du magnésium."[1]
again observed the phenomenon in 1873 :—

> "Tout récemment, j'ai eu l'occasion de constater la p
> magnésium sur le bord entier, plusieurs fois de suite, du
> juin. La correspondance presque exacte des dates semb
> une influence de la déclinaison de l'astre ; mais, en discu
> les observations, j'ai déjà démontré, dans les *Memorie*, que
> bilité du magnésium correspond à des conditions spéciales d
> du Soleil."[2]

Tacchini also states that the luminosity of the *b*
in direct relation with the brightness of the chro
and that 1474 also appeared ; and hence sta
magnésium était donc abondamment répandu, a
dans toute la chromosphère, mais avec une inten
variable, et sans rapport marqué avec les pôles du

He gave a curve showing the intensities of the
the solar disc.[3]

NOTE P.

Solar Outbursts and Magnetic Sto

NOTE P.

In connection with the solar storm of April 2
(see page 236), I wrote to Dr. Balfour Stew
asked him if he had observed any correspon
turbance of the magnets of the Kew Observat
sent me a sun picture for the day, and stated t
was no disturbance.

Young has, however, since persisted in the inq
records several cases of coincidence between s
magnetic storms, and to him belongs the full
calling attention to the possible connection.

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[1] *Comptes Rendus*, vol. lxxv. pp. 24 and 25.
[2] Op. cit., vol. lxxiv. p. 1577, June 30, 1873.
[3] Op. cit., vol. cit., p. 1578, *et seq.*

NOTE P.

His first communication appears in the *Boston Journal of Chemistry*,[1] and relates to a storm in September 1871. The following is a later communication :—[2]

"A careful comparison of some of these observations with the corresponding magnetic records at Greenwich and Stonyhurst, for copies of which records I am indebted to the courtesy of Sir G. B. Airy and Rev. S. J. Perry, goes far to show that, although probably the *greatest* magnetic disturbances are due to terrestrial causes, or at least are only indirectly results of solar or cosmical influences, yet, on the other hand, every solar paroxysm does have a distinct, direct, and immediate effect upon the magnetic elements. Thus on August 3 such solar paroxysms were noted at 8.45, 10.30, and 11.55, also on August 5 from 6.20 to 7.30 A.M. (Sherman time), and the last was the only outburst during the day.

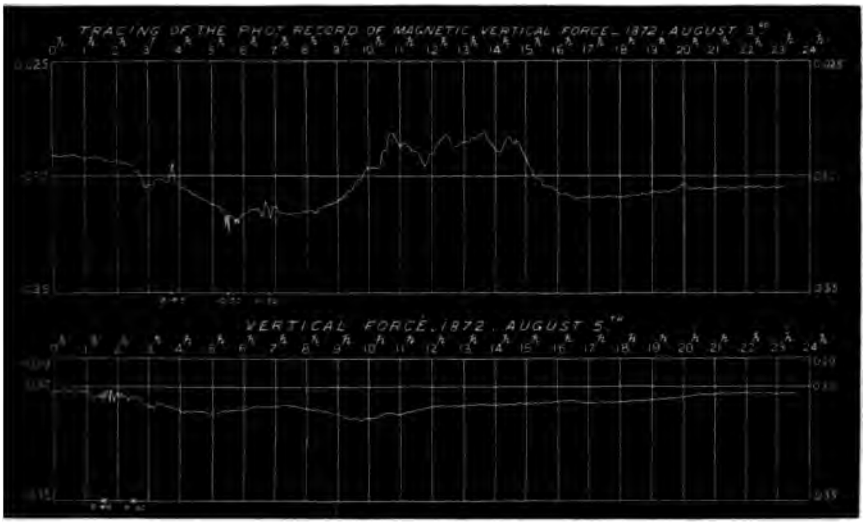

FIG. 175.—Diagrams showing magnetic storms coincident with solar outbursts.

"Now the annexed figure (Fig. 175), from a photographic copy of the vertical force curve for these days at Greenwich, shows marked and characteristic disturbances at the points indicated, which, allowing for the longitude, correspond to the very instants when the solar disturbances were noted. Further comparisons of such phenomena will be necessary to establish the conclusion with absolute certainty ; but in the meantime it seems altogether probable that every solar disturbance

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[1] Vol. v. 1871, p. 68.

[2] *Nature*, vol. iii. p. 107, December 12, 1872.

NOTE P.

receives an immediate response from the earth, and that the
impulse travels with, sensibly, the velocity of light."

The following letter from the Astronomer Royal
interesting question :—

"In the French *Comptes Rendus* of August 4, which
reached this country, is an account by Father Secchi of a re
outburst from the sun's limb witnessed by him on July 7, wh
from 3 h. 30 m. to 6 h. 59 m. (Roman time, I presume),
2 h. 40 m. to 6 h. 0 m. (Greenwich time).

"A magnetic storm commenced at Greenwich at 5 h. 0 m. pr
the same day. Its indications began at that time with unusu
ness and strength, on all the magnetic indicators—namely, th
tion needle, the horizontal force magnetometer, the vertical f
netometer, the earth-current wire, in an approximate N.W.
direction, and on the earth-current wire in an approximate N
S.E. direction. The disturbance lasted, gradually diminishi
evening of July 9. During a part of the time it was accompa
aurora.

"I do not venture upon the question whether there reall
connection between the solar outburst and the terrestrial
storm, but I will remark that, if there was such connection,
mission of the influence from the sun to the earth must have
2 h. 20 m. ; or a longer time, if Father Secchi did not see the r
ing of the outburst. This, if established, would be an i
cosmical fact ; and, at any rate, the notification of this
retardation may direct the attention of observers of similar ph
in future to a new element in their interpretation."[1]

NOTE Q.[2]

SIMPLIFICATION OF SPECTRA BY REDUCTI
PRESSURE.

NOTE Q.

On the 5th of May, 1869, Dr. Frankland and
communicated our first results on the simplific
spectra by reduced pressure, which have since b
lowed up with important consequences (see p. 53
we pointed out that this might be the cause of the
spectra of the nebulæ.

The observations recorded on p. 530 were thus
to by Sir William Thomson as President of the
Association at the meeting in Edinburgh in 1871.

[1] *Nature*, vol. vi. p. 328, August 22, 1872.
[2] See p. 530.

NOTE Q.

"Stokes's dynamical theory supplies the key to the philosophy of Frankland and Lockyer's discovery. Any atom of gas when struck and left to itself vibrates with perfect purity its fundamental note or notes. In a highly attenuated gas, each atom is very rarely in collision with other atoms, and therefore is nearly at all times in a state of vibration. Hence the spectrum of a highly attenuated gas consists of one or more perfectly sharp bright lines, with a scarcely perceptible continuous gradation of prismatic colours. In denser gas each atom is frequently in collision, but still is for much more time free, in intervals between collisions, than engaged in collision; so that not only is the atom itself thrown sensibly out of time during a sensible proportion of its whole time, but the confused jangle of vibrations in every variety of period during the actual collision becomes more considerable in its influence. Hence, bright lines in the spectrum broaden out somewhat, and the continuous spectrum becomes less faint. In still denser gas each atom may be almost as much time in collision as free, and the spectrum then consists of broad nebulous bands crossing a continuous spectrum of considerable brightness. When the medium is so dense that each atom is always in collision, that is to say never free from influence of its neighbours, the spectrum will generally be continuous, and may present little or no appearance of bands, or even of maxima of brightness. In this condition the fluid can no longer be regarded as a gas, and we must judge of its relation to the vaporous or liquid states according to the critical conditions discovered by Andrews."

Nevertheless Mr. Huggins saw in our results no new idea or discovery, but simply a plagiarism on his own work, in which there had been no variation of pressure or passage from a dense gas to a rare one, or *vice versâ*, at all; and on the publication of our paper he addressed the following letter to the editor of the *Chemical News*:—[1]

"GASEOUS SPECTRA.

"SIR,—In the paper on 'Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun, Stars, and Nebulæ,' by Dr. Frankland and Mr. Lockyer, as reported by you, the authors do not notice similar observations which I presented to the Royal Society in April 1868. I send you the following extracts from that paper, which is printed in the *Phil. Trans.* for 1868.—I am, &c.,

"WILLIAM HUGGINS.

"'The following observations are suggestive in connection with the point under consideration. Electrodes of platinum were placed before the object-glass in the direction of a diameter, so that the spark was as nearly as possible before the centre of the lens. The spark was taken in air. I expected to find the spectrum faint, for the reasons which have been stated in a previous paragraph (the diminution of the

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[1] *Chemical News*, July 9, 1869.

NOTE Q.

brightness of the spark from the distance at which it is p
the slit), but I was surprised to find that only one line was
the large spectroscope when adapted to the eye-end of the
This line was the one which agrees in position with the
nebula, so that under these circumstances the spectrum o
appeared precisely similar to the spectra of those nebula
the light is apparently monochromatic. This resemblance
more complete by the faintness of the line; from whic
appeared much narrower, and the separate existence of its t
nents could no longer be detected. When this line wa
simultaneously with that in the nebula, it was found to ap
very little broader than that line. When the battery c
completed, the line from the spark coincided so accurately
with the nebular line that the effect to the eye was as if
increase of brightness in the line of the nebula had taken
order to make this observation, and to compare the relati
ance of the lines, the telescope was moved so that the ligh
nebula occupied the lower half only of the slit. The line o
was now seen to be a very little broader than the line of
and appeared as a continuation of it in an unbroken st
These observations were repeated many times on several
(P. 542.)

"'The double line in the nitrogen spectrum does not
sharply defined lines, but each component is nebulous, and
a greater width than the image of the slit.[1] The breadt
lines appears to be connected with the conditions of tens
temperature of the gas. Plücker[2] states that when an indu
of great heating power is employed, the lines expand so
and form an undivided band. Even when the duplicity exi
ceases to have the power to distinguish the component l
intensity of the light be greatly diminished.

"'Though I have been unable to detect duplicity in the c
ing line in the nebula, it might possibly be found to be dou
under more favourable conditions: I incline to the belief t
double.[3]

"'In my tables of the lines of the air,[4] I estimated the br
each of the components of the double line in the spectrum
at 10, and the components of the double line next in br
the orange at 7 and 5, and those of a third double line
refrangible side of D at 6 and 4. It was with reference t
double lines next in apparent brilliancy that I wrote,[5] in s
the line in the nebula, "If, however, this line were due to n
ought to see other lines as well; for there are especially

---

[1] Secchi states that with his direct spectroscope this
annular nebula in Lyra appears double. As the image of
is viewed directly, after elongation by the cylindrical lens,
a slit, it is probable that the two lines may correspond to th
of the elongated annulus of the nebula.

[2] *Phil. Trans.* 1863, p. 13.

[3] "On the Spectra of the Chemical Elements," *Phil. T*
p. 141.

[4] *Ibid.*

[5] *Ibid*, p. 443.

double lines in the spectrum of nitrogen, one at least of which, if they existed in the light of the nebulæ, would be easily visible."

"'As the disappearance of the whole spectrum of nitrogen, with the exception of the one double line, was unexpected, though, indeed, in accordance with my previous estimations, I examined the spectrum of nitrogen with a spectroscope furnished with one prism with a refracting angle of 60°, in which the whole of the spectrum from C to G is included in the field of view. I then moved between the eye and the little telescope of the spectroscope a wedge of neutral-tint glass corrected for refraction by an inverted similar wedge of crown glass, and which I had found to be sensibly equal in absorbing power on the different parts of the visible spectrum. As the darker part of the wedge was brought before the eye, the two groups in the orange were quite extinguished, while the lines in the green still remained of considerable brightness. The line which under these circumstances remained longest visible next to the brightest line, was one more refrangible at 2669 of the scale of my map. This observation was made with a narrow slit. When the induction spark was looked at from a distance of some feet with a direct-vision prism held close to the eye, I was surprised to observe that the double line in the orange appeared to me to be the brightest in the spectrum, and when the neutral-tint wedge was interposed, this line in the orange remained alone visible, all the other lines being extinguished.

"'When, however, in place of the simple prism a small direct-vision spectroscope provided with a slit was employed, I found it to be possible, by receding from the spark, to find a position in which the double line in the green, with which the line in the nebula coincides, was alone visible, and the spectrum of the spark in nitrogen resembled that of a monochromatic nebula.

"'It is obvious that if the spectrum of hydrogen were reduced in intensity, the line in the blue, which corresponds to that in the nebula, would remain visible after the line in the red and the lines more refrangible than F had become too feeble to affect the eye.

"'It therefore becomes a question of much interest whether the one, two, three, or four lines seen in the spectra of these nebulæ represent the whole of the light emitted by these bodies, or whether these lines are the strongest lines only of their spectra which, by reason of their greater intensity, have succeeded in reaching the earth. Since these nebulæ are bodies which have a sensible diameter, and in all probability present a continuous luminous surface, or nearly so, we cannot suppose that any lines have been extinguished by the effect of the distance of these objects from us.

"'If we had evidence that the other lines which present themselves in the spectra of nitrogen and hydrogen were quenched on their way to us, we should have to consider their disappearance as an indication of a power of extinction residing in cosmical space, similar to that which was suggested from theoretical considerations by Chéseaux, and was afterwards supported on other grounds by Olbers and the elder Struve. Further, as the lines, which we see in the nebulæ are precisely those which experiment shows would longest resist extinction, at least so far as respects their power of producing an impression on our visual organs, we might conclude that this absorptive property of

NOTE Q.

space is not *elective* in its action on light, but is of the char
general absorption acting equally, or nearly so, on light
degree of refrangibility. Whatever may be the true sta
case, the result of this re-examination of the spectrum of th
appears to give increased probability to the suggestion tha
from my former observations, namely, that the substances
and nitrogen are the principal constituents of the nebulæ of
under consideration.' "—(Pp. 543, 544.)

To this Dr. Frankland and myself sent the f
reply:—

"GASEOUS SPECTRA.—*To the Editor of the Chemical N*

"SIR,—Our attention has been drawn to a letter from
Huggins, in your impression of the 9th instant, stating t
recent note to the Royal Society (in which we announced
varying the conditions of pressure and temperature, we had
the spectrum of hydrogen, as seen in our instrument, to
absolutely, and the spectrum of nitrogen to one line nearly
not notice similar observations of his own presented to t
Society in 1868. In reply, we beg to state—first, that we
Mr. Huggins' observations, and, secondly, that they were n
to our own.

"With regard to the first point. The account you were goo
to publish contained the following:—The bearing of these
servations on those made on the nebulæ by Mr. Huggin
Secchi, and Lord Rosse is at once obvious. The visibility o
line of nitrogen has been taken by Mr. Huggins to indicate f
first, 'a form of matter more elementary than nitrogen, and
analysis has not yet enabled us to detect;' and then, sec
power of extinction existing in cosmical space.'

"'Our experiments on the gases themselves show not only
assumptions are unnecessary, but that spectrum analysis here
us with a means of largely increasing our knowledge of the
constitution of these heavenly bodies.'

"We thus gave briefly the results of Mr. Huggins' inqu
references to where a complete account of his observations w
found. This we considered to be sufficient, seeing that our c
cation was a preliminary note only. In the complete accou
researches, to be subsequently presented to the Royal So
shall, of course, dwell upon them at greater length.

"With regard to the second point—the similarity of the
tions. In 1864, Mr. Huggins first observed the spectrum
nebulæ, and determined that one of the lines was coincident
of the air lines due to nitrogen, the spark being taken close to
troscope. On this Mr. Huggins wrote:—'The speculation
itself, whether the occurrence of this one line only in the neb
not indicate a form of matter more elementary than nitro
which our analysis has not yet enabled us to detect.' S

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[1] *Chemical News*, July 30th, 1869.

afterwards, Mr. Huggins, instead of taking the air-spark close to the spectroscope, placed it outside the object-glass of his telescope, and then only saw one line in the spectrum—the line in question. He next got similar results close to the spark by interposing a dark glass to cut off the light, still using the spark in air at the ordinary pressure, and remarked that it was 'obvious' that the hydrogen spectrum would give similar results. Then, after stating 'we cannot suppose that any lines have been extinguished by the effect of the *distance* of these objects from us,' he refers to the possibility of a 'power of extinction residing in cosmical space,' and the hypothesis of the more simple form of nitrogen appears to be given up.

"Now, with regard to our own observations, we have *not* taken the spark in air; we have *not* used a dark glass; we have *not* worked at atmospheric pressure; nor have we disturbed the normal distance of the experimental tube from the slit of our instrument until we have reduced the complicated spectrum of nitrogen to one bright line and three or four other extremely faint ones. Further, we have *not* rested content with any assumption with regard to hydrogen, nor have we regarded anything as obvious.

"What we have done is to carefully watch both spectra changing as the pressure becomes less, trying the effect of increased and decreased temperature at every stage under various conditions, which we shall state in our detailed paper.

"It may be as well to refer, in conclusion, to Plücker's important work on nitrogen, which shows that, under certain conditions, the green line of nitrogen vanishes altogether from the spectrum. This fact alone shows at once the importance of our method of inquiry.

"We are, &c.,
"E. FRANKLAND.
"J. NORMAN LOCKYER.

"*Royal College of Chemistry, Oxford Street,*
"*July* 23, 1869."

In this case, as in the former one, Mr. Huggins has neither substantiated his statement nor withdrawn it. Instead of this, he has reiterated it,[1] taking no notice of our reply!

After the notes on the function of pressure had appeared in the *Proceedings of the Royal Society*, Herr Wüllner published a memoir on the subject, in which he ascribed the changes observed to temperature. He has since, however, changed his opinion, and now supports the view put forward by Dr. Frankland and myself. M. Salet, however, ascribes these changes to temperature merely.[2]

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[1] See *Proc. R.S.*, vol. xx. p. 383, Note.

[2] Thesis read before the Faculty of the University of Paris, p. 18.

NOTE Q.

More recently, Schuster[1] has discussed the a
electrical resistance in these terms :—

> "We cannot alter the pressure of a gas without altering it
> resistance, and, therefore, also the strength of the electric cu
> the heat developed. We can only decide the question by s
> the gas at the *same* temperature to different pressure. Now h
> ever been any such experiments made? I think there have,
> very decisive ones. Frankland and Lockyer have found
> increase the pressure of hydrogen while an electric current i
> through it, the lines begin to expand till the spectrum beco
> tinuous, and finally the resistance becomes so large that th
> current will not pass at all. On the other hand, Gassiot an
> have observed that, if we diminish the pressure of hydrogen, i
> resistance force diminishes, attains a minimum, then increa
> and if we keep on exhausting the tube, it becomes again so
> the current cannot pass. Plücker says that a tube exhaus
> utmost limits shows the lines of hydrogen and silica. He
> at one place, I think, that the lines are very fine and di
> there would have been any widening he would have bee
> mention it. Now it is not too much to assume that the res
> the gas at the moment when the discharge just ceases to tak
> the same whether the increase of resistance is produced by
> a pressure or too great an exhaustion. At this moment, the
> current is the same, and the same energy must be converted
> by resistance. But in the case in which the current does n
> account of the excessive diminution of pressure, only a mu
> quantity of gas has to be heated than in the other case.
> therefore, be heated up to a much higher temperature, an
> spectrum is not continuous and the lines are not even wide
> are, therefore, compelled to accept Frankland and Lockyer
> conclusion, that pressure and not heat is the cause of the w
> the lines.
>
> "The question is one of considerable importance. If te
> would widen the lines, the widening ought always to begin a
> temperature, and the hydrogen in the solar protuberances w
> only narrow lines could not be at a higher temperature
> hydrogen in our vacuum tubes, the moment the lines begin
> If our conclusion, however, is correct, the breadth of the
> give us no indication whatever as to the temperature of the

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[1] *Nature*, vol. viii. p. 496.

APPENDICES.

I.—*INSTRUCTIONS TO OBSERVERS OF THE ECLIPSE OF 1871.*

II.—*PROFESSOR RESPIGHI'S MEMOIR ON THE FREQUENCY AND DISTRIBUTION OF THE PROMINENCES AND THEIR PERIODICAL VARIATIONS.*

APPENDIX I.

INSTRUCTIONS TO OBSERVERS OF THE ECLIPSE OF 1871.

I.—*Instructions for the Polariscopic Observations of the Corona, including Beams and Streamers.*

APPENDIX I.

IT is recommended that the polariscopic examination of the corona be carried on as follows:—

1st.—To examine a detached and selected part of the corona about 6′ from the limb of the sun, and say about 8′ in diameter.

2nd.—A field extending from the limb of the sun outwards should be examined either with a Nicol's prism or a double-image prism.

3rd.—The light of the streamers at some distance from the sun should be examined with a Nicol and a crystal.

4th.—The polarization of the corona should be examined in such a manner as to eliminate atmospheric polarization.

NOTE.—The most suitable instruments for ascertaining the plane of polarization and the proportion of polarized to unpolarized light are,—

(1) A double-image prism.

(2) Savart's polariscope.

(3) A plate of quartz consisting of two compensating wedges.

(4) A plate of Arragonite, or calc spar, cut perpendicular to an optic axis, and affixed to an analysing prism.

(5) A polarimeter consisting of four plates of glass, movable on an axis perpendicular to the plane of polarization.

(6) A compound plate of right-handed and left-handed quartz.

First Observation.—The object of this observation is to observe the polarization (if any) of the corona without having the observer's attention distracted by the chromosphere. A Savart's polariscope is recommended by preference. The Nicol's prism of the polariscope should be set beforehand with its principal plane (or plane of symmetry) radial, *i.e.* perpendicular to the sun's limb, and the observer must note whether bands are visible; and if so, whether they are black-centred or white-centred. Should the bands be feeble, it will be well to rotate the polariscope, prism and plates of course moving together, and quickly restore it to its primitive azimuth, after having noted the estimated azimuth of the Nicol when the bands are strongest and black-centred. Should no bands or only dilute bands be seen, it may be that the corona, though polarized, is overpowered by other light; and the observer will move the telescope from the sun, radially if it may be; if in any other direction, rotating the polariscope so as to

APPENDIX I.

render its principal plane radial in the new position. He
note whether, though the light becomes feebler, the bands b
dilute.

Should, on the other hand, the bands be strong, after havi
himself as to the plane of polarization, the observer will en
make out whether by means of the polarization of its light
can be detected superposed on the chromosphere. He will
move the telescope *towards* the sun, keeping the principal p
polariscope radial, and note how near the sun the band
be seen. To provide for the contingency of this observation
well to point in the first instance to the side of the sun wh
first uncovered.

If time permit, he should try whether there is any sensib
of polarized light on the dark disc of the moon, rotating th
and determinating the plane of polarization.

Second Observation.—The special object of this observa
differentiate, if possible, between the corona, on the one han
chromosphere, or whatever else may be self-luminous (t
a portion of the corona itself), on the other. This will b
if the light of the corona be strongly polarized, so as to
comparative extinction by an analyser. The observer shoul
analyser so as to extinguish, as far as may be, the light of t
in the neighbourhood of a radius depending on the angular
the analyser. He should notice the form, colour, and gener
ance of any residual luminosity other than the well-known
ances ; should contrast the appearance, especially as to co
that seen when the light is analysed so as to retain light from
region polarized in the perpendicular direction, and should a
whether the luminosity is such as could be accounted f
superior brightness near the sun of the unanalysed light, even
were to suffer the same proportionate loss by analysis as the
a greater distance.

Of the instruments named, the Nicol's prism is better ada
general survey, not requiring any limitation of field ; the dou
prism is better adapted for a comparison of the oppositely
images, since the observer can compare them *directly*, not on
memory of the other. The double-image prism will require a c
with a long and moderately broad aperture in the focus o
piece, rotating with the prism, and in the observation the len
be placed in a radial direction.

Third Observation.—The observation of the streamers as
zation might throw much light on their nature, and the obs
undertakes this observation, by means of a Savart's polar
else of a Nicol's prism capped with a plate of calc spar or a
will be in a condition to determine with advantage the plane
zation, if there be polarization, of the corona generally.
streamers must be his first object.

Fourth Observation.—It has been supposed by some
polarization observed in the corona was really due to the
illumination of the intervening portion of the earth's atmos
other words, to the illumination produced by reflexion fro
&c., towards the horizon. This effect may be eliminated

a Savart's polariscope, or, better, a polariscope with quartz wedges, and turning the instrument till the bands (if any) seen on the moon's disc disappear. The corona can then be scrutinized as to polarization, and the polarization examined in different azimuths of the Nicol's prism relative to the radius drawn from the sun's centre, by pointing the telescope instead of rotating the analyser. In this observation the observer has the choice of two rectangular azimuths of the polariscope, for each of which the bands (if any) on the moon disappear; and if no bands be seen on the moon he is free to scrutinize the polarization of the corona by turning the polariscope.

The atmospheric polarization may also be very conveniently eliminated by means of a bi-quartz polariscope, which is previously set, so that the line of junction shall mark the plane of polarization of light which gives the two halves purple alike, and which has the right-handed[1] half marked. The instrument is turned till the two halves seen on the dark moon are coloured alike (whether purple or yellowish to be specified), and the colours of the *extremities* of the segments of the corona contiguous to the line of junction are to be observed, both when the corona is bisected and divided unequally. The polariscope should then, if time permit, be turned through 90°, so as again to get the segments of the dark moon coloured alike, and the observation repeated.

General Remarks.

The object-glasses of all telescopes intended to be used in polariscopic observations should be examined before departure as to their freedom from defects of annealing.

All polariscopes including a Nicol's prism, or tourmaline, should be marked, so that the principal plane may be readily known *by feeling*, as sight-marks might fail for want of light. Double-image prisms should have one side of the aperture in the diaphragm marked so as to distinguish the two images.

Each observer will be held responsible for the proper marking of his own instrument, and for the correct description of the relation of the marks to the instrument, and to the phenomena observed by means of it.

II.—*Instructions for the Spectroscopic Observation of the Chromosphere and base of Corona.*

NOTE.—The objects to be obtained are :—

1. To determine the actual height of the chromosphere as seen with an eclipsed sun; that is, when the atmospheric illumination, the effect of which is doubtless only partially got rid of by the Janssen-Lockyer method, is removed. If the method were totally effective, the C line, the line of high temperature, should hardly increase in height; but there can be little doubt that the method is not totally effective, so the increase in height should be carefully noted.

2. To determine if there exists cooler hydrogen above and around the vividly incandescent layers and prominences. To do this the band of the spectrum just above the stratum, which gives the hydrogen lines before totality and during totality, should be carefully examined, to

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[1] N.B. *Not* right-hand half.

notice (*a*) if any traces of the hydrogen spectrum exist above t
which before totality gave the hydrogen lines, and (*b*) w
extend outside the hydrogen spectrum, and whether they
with it in the lower strata.

3. To test the American observations of last year a
existence of a line at 1474 in the corona spectrum, by
it be visible above the region which gives the hydrogen spect

4. To determine whether any other gases or vapours are
mixed up with hydrogen, but remain invisible with the uneclips
consequence of the absence of saliently brilliant lines in thei

The observations should be conducted as follows :—

1. Work with a horizontal slit, or a slit in a parallel of de
according to the instrument used, whether altazimuth or equ

2. See that the spectroscope-telescope works easily, so that
along the spectrum is easy.

3. Find before totality an average plain-topped regio
chromosphere, where some motion on either side does not br
thicken, or lengthen the lines near the part covered at the co
ment of totality.

4. Observe this before and during part of totality, the
being driven by the clock if an equatorial is used.

5. Just before totality sweep from red to violet ; note the
their lengths ; mark the 1474 line and the lines between
carefully.

6. Immediately after the commencement of totality swee
note new lines—their heights ; especially the comparative h
C, near D, F, and near G, with these former heights ; and
whether band over *b*, and the thickness of F. Note also
between D and 1474.

7. Just before the end of totality unclamp and bring back t
the *following* limb of the moon ; and note the extinction of
trum by the reappearance of the sun, if possible by a rapid s
this is not possible, then watch the behaviour of F ; sweep ba
to see if there be any variations from (5) in the new reg
observed.

8. Carefully note position angle where slit cuts limb.

9. Record the impressions of facts, and facts not already
soon as possible.

III.—*Instructions for the Spectroscopic Observation of the*

NOTE.—The word corona is here used for convenience, t
all the light above the prominences. It therefore includes th
the chromosphere which is generally veiled to us when obs
are made by the Janssen-Lockyer method.

The principal object to be obtained is :—

To determine whether it is possible to differentiate the out
of irregular outline and the streamers from a stratum say som
high round the sun, which may possibly be the limit of the
envelopes above the photosphere.

To attack this question, we require a long slit, a large
a long collimator, with large angular aperture and small d

power. The slit must be adjusted on a faint cloud before totality, and on no account is it to be touched before observations of a similar cloud can be made after totality, by the head of the party.

The most important observation to make is, whether there are any dark lines in the spectrum at any distance from the sun; and if so, at what distance, and their exact positions.

Next, whether there are any bright lines; if any, their positions must be noted; it must be especially seen whether the lines recorded by the American observers are again visible.

The observations should be conducted as follows:—

1. In each party one observer should arrange the instrument so that the image of the following limb of the moon, at the point of its first contact, will fall on the left-hand side of the slit, *placed nearly horizontally*.

2. See if corona is visible before totality. When visible, note its spectrum with the utmost care. Note whether the spectrum is continuous or discontinuous. Note especially, if bright lines are visible, whether they are all of equal height.

3. After this, if there is still time, move the instrument in azimuth, so that the chromosphere on the preceding side is visible; this may settle some doubtful lines. Then observe the corona after totality, if possible.

Another observer should work with a vertical slit, and observe the corona above and below the moon.

IV.—*Some Particulars to be especially noticed by those Observers who make Drawings of the Corona.*

1. Its extent. If there is no definite boundary, this *should be stated*.

2. Whether there is any change. (All changes must be most carefully shown in any manner the artist may prefer).

3. Whether there are any streamers.

4. All tints and changes of tint, and whether the colour is distributed in patches, or disposed in any geometrical form, or whether it exhibits any particular relation to the prominences.

5. Whether it consists of a level patch of luminous haze, or radiating beams of light, or of pairs or bundles of hyperbolic rays.

6. If of radiating or other beams, whether they are evenly distributed all round, or in groups only.

7. Whether the dark intervals between such radiating beams are constant or fluctuating.

8. Whether it is concentric with the moon.

9. Whether it is equally intense all round the moon.

10. Whether the outer border exhibits any coruscations, or whether its definition is permanent and equally pronounced all round.

11. Whether the light of the corona is more intense or less so in the immediate neighbourhood of the prominences.

12. How much darker the moon's disc is than the sky outside the corona.

13. Whether the fading is sudden or gradual from the moon's limb.

APPENDIX II.

PROFESSOR RESPIGHI'S MEMOIR ON THE FREQUENCY OF TUBERANCES AND THEIR PERIODICAL VARIATIONS; A DISTRIBUTION OF THE PROTUBERANCES OVER THE SU THE SUN, AND THEIR PERIODICAL VARIATIONS.[1]

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SECTROSCOPIC observations show us the protuberances
really on the solar circumference, and more than this, all
are on one or on the other side of the limb which ar
enough for their summits to project above it. As the heig
protuberances are very variable, and their distribution ove
surface rather irregular, it is impossible to form a corre
the number of them which exists at any moment on the
the sun.

Nor does the rotation of the sun furnish us with a m
making this computation, on account of the diversity of hei
protuberances, their different persistencies, and the various
of their visibility in correspondence with their polar distance

The computation would be less difficult for the small prot
which, on account of the rotation of the sun, do not remain
more than one day, since we could obtain their totality from
sum of the number of those which were observed on the days
the entire rotation, or from the average of the diurnal numbers
by 27·3, the mean duration of the rotation; but among thes
rances are also others much higher, visible only at their sum

By a calculation which I made of the total number of th
rances under the height of 30″, without taking into acc
small jets which were on the surface of the sun during t
of the greatest frequency (and this was towards the end of
there would not be so many as 400: while in the period of
frequency (which would correspond to the beginning of
number would have been below 250, supposing the protube
be uniformly distributed over the surface of the sun, or
several meridians. With regard to the large protuberance
those not less than 1′ or more than three terrestrial dia
height, taking as a basis the number of these protuberances
during the whole period of observation in the several sol
and supposing that from the pole to 10° polar distance the
rances higher than 1′ are all visible on the same day, and th
equator, on the other hand, one of these protuberances may
for one day only, by reason of the solar rotation; and s
besides, that in every zone or parallel they are distribu
uniformity; I find that, in an average result, the number of
protuberances all over the surface of the sun is below twenty.

These numbers must, however, be considered as only
approximative, and then even scarcely sufficient to give
probable idea of the total sum of these centres of eruptio
solar surface.

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[1] Extract from his 5th Note.

Although from the number of the protuberances which we may observe every day on the solar outline, we cannot form a correct idea of the total number of those that are on the solar surface, still we can, from the average of the diurnal results, obtained by me from a continued series of days, get a sure indication of the greater or less frequency of the phenomenon, and so of the greater or less activity of the sun itself. Starting from these data, it is found that the frequency of the phenomenon is variable in different parallels, in different solar meridians, and at different epochs.

Dividing the entire period of observation into five partial Periods, we have :

1st from Oct. 26, 1869, to April 30, 1870.
2nd „ May 1, 1870, „ Oct. 31, 1870.
3rd „ Nov. 1, 1870, „ April 30, 1871.
4th „ May 1, 1871, „ Sept. 1, 1871.
5th „ Jan. 29, 1872, „ April 30, 1872.

And dividing the total number of the protuberances observed in each of them by the number of days of observation, or by the number of profiles taken of them, it is found that the diurnal averages of the protuberances observed during each period are :

1st	Period,	number of	Protuberances	21
2nd	„	„ „	„	24
3rd	„	„ „	„	20
4th	„	„ „	„	37
5th	„	„ „	„	31

Although these numbers have not been deduced from periods of equal duration, and from an equal number of days of observation, they are enough to give us some idea of the greater or less frequency of the phenomenon in the several epochs taken into consideration ; and from them we may conclude that, from the first period of observation to the fourth, there is a progressive increase in the frequency of the protuberances, while from the fourth to the fifth there is a sensible and gradual decrease.

In the third period of observation, this progressive increase in the diurnal number of the protuberances was not observable ; but this might be attributed to the circumstance that, during that period, the weather was very unfavourable to these observations ; so that while I very seldom succeeded in obtaining the number of complete profiles, some of them which did succeed were not satisfactory in their details, and certainly many protuberances were missing, which on account of their inconsiderable height or their faint light could not be brought out decidedly, by reason of the abnormal conditions of the atmosphere.

Between the fourth and fifth periods we have an interruption of five months, during which, according to the observations made by Father Secchi, there was a minimum of protuberances, after which the solar activity was resumed and invigorated, but did not attain the maximum arrived at during Period IV. This variation of frequency in the protuberances corresponds, then, to the successive variations in the frequency of the solar spots observed precisely at this period ; and this furnishes a proof of the close relations existing between

the two kinds of phenomena ; relations which will be
manifest in the following paragraph.

In the first Note which I published on my spectroscopi
tions of the solar protuberances (which was read at the
Academy of the *Nuovi Lincei*, at the meeting of Dec.
in speaking of the distribution of the protuberances over
surface, I say (at page 13) :—

"Concerning the distribution of the protuberances ove
surface, it is found that they are more frequently pres
zones of the spots, or rather of the faculæ, and generally ne
but without being confused or blended with them. It app
that the protuberances approach nearer to the poles than
but this may be simply an illusion, depending on the d
seeing the faculæ in the more immediate vicinity to the pole

"From the figures presented in the accompanying table
from many others taken near the poles on other days in the
of calm, the following law is clearly arrived at ; that in t
of the solar poles, through a distance of about 20° from
phenomenon of the protuberances either does not take p
occurs only to a very small and exceptional degree.

"In fact, during a period of more than forty days, no
tuberance was seen within those limits ; on three days
small jets were observed, and these last but for a very short

"This law, which would have been deduced with difficul
the observation of a long series of total eclipses, clearly s
the protuberances are, in relation to the rotatory motion o
like the spots and the faculæ."

From this it was manifest to me, from the time of my
servations and studies of protuberances, that these phen
not produced casually and indiscriminately in the differen
the solar surface, but that their distribution is to a mark
subordinated to the solar rotation, and strictly depende
condition of the axis of rotation.

This result was confirmed by me in my Note II. (re
meeting of April 3, 1870, of this Academy), and again
plained and developed in a more detailed manner in the N
Dec. 4, 1870 ; in which, taking for my basis the observati
from Oct. 26, 1869, to the end of Nov. 1870, I deduced the
results with regard to the distribution of the protuberance
surface of the sun :—

I. In the circumpolar regions (namely, for about 20°
poles) the phenomenon of the protuberances or solar er
either never observed, or seen quite exceptionally, occurrin
localities only as little jets and of short duration. In the
also, the red stratum and the solar surface cannot be r
a state of perfect calm, because of the presence of the s
and the more or less marked irregularity of the stratu
a state of continual agitation, and of continual, although
eruption.

II. That in the equatorial zone to the extent of about 20
tuberances are less frequent and less developed than in th
higher latitudes.

III. That the phenomenon seems more frequent in the northern hemisphere than in the southern hemisphere.

IV. That in the northern hemisphere the large protuberances—that is, those of not less height than three terrestrial diameters—are more frequent in two zones, one extending from 10° to 20° degrees of latitude, the other from 60° to 70°, and are less frequent in proximity to the equator, and in the zone included between 30° to 40° of latitude.

V. That in the southern hemisphere, the frequency of the large protuberances is nearly constant in the zone included between 20° and 60°, then decreases rapidly as we approach nearer to the equator and to the poles.

VI. That the large protuberances are met with at the northern pole more than at the southern; so that during the period of observation, while some protuberances are observed in the former hemisphere, on the other side of the parallel of 70° of latitude, during the second period, on the other hand, they are seldom seen on the other side of the parallel of 60°.

VII. In the higher latitudes, the predominant form in the protuberances is that of large groups of delicate jets or light threads, not very luminous, and more or less irregularly intermingled.

VIII. That the protuberances nearer to the poles are less subject to transformations, and are much more persistent and durable than those which occur in the lower latitudes.

I remarked, moreover, that the notable approach of the large protuberances to the north pole is observed chiefly during those periods in which there were great magnetic perturbations and remarkable auroræ boreales.

In expounding these results, I had no intention of establishing definite and unchangeable laws concerning the distribution of the protuberances over the solar surface; I merely proposed to show the relation of these phenomena to the rotation of the sun, and their dependence on it, leaving the determination of definite laws to a more prolonged and regular series of observations.

That thus (considering these phenomena to be closely connected with those of the faculæ and the spots, and that during the intervals of the observations some marked periods of greater or less frequency had been presented in the different parts of the solar surface) in paragraph 6 of Note III. (to which I have referred) I pointed out the periodicity of these phenomena, and the alternations of maximum and minimum, as with the spots; remarking however, that such investigations could not be undertaken without a prolonged series of observations. From what I have explained, it is evident that from my observations, not only had I deduced that the distribution of the protuberances over the solar surface was connected with the rotation of the sun, but more than this—I had clearly formulated the relations existing between them during the period of observation.

Reca, then an illustrious Italian astronomer, several months after the publication of this last Note of mine, announced as a discovery derived from his own observations, the fact, that the distribution of the protuberances over the solar surface is not accidental and casual, but in subordination to the solar rotation; publishing this

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discovery in almost the same terms as those in which I l
enunciated this fact as the result of former observations of
It was under these circumstances that, at the end of l
published my Note IV., not so much for the purpose of sl
the professed discovery was already of old date, as to m
that if the relation between the distribution of the protul
the solar surface, and the position of the axis of rotation
is not to be doubted, yet the definite laws of this relatio
be established, except by observations extending over a lo
time, and corresponding probably to the period of the ma
minimum of the solar spots ; because the observations al
manifestly showed that the protuberances, in their frequ
size, and their distribution over the several zones, were
periodical alteration analogous to that of the spots. Takin
of the long space of time embraced by my observations,
coincidence with the period of the maximum of the spot
in that Note how, with the progressive increase of the solar
was also a progressive increase in the total number of
berances, a noticeable increase in the frequency of the l
berances, and a very marked condensation of them in the p
in which, before these phenomena were observed, they v
totally absent, at least rare and decidedly exceptional.
ceedingly important results could not be obtained by the
made in the year 1871, because they were coincident wi
of extraordinary perturbation, depending on the period c
mum of the solar spots ; and thus it is that some astronon
professed to deduce from these observations the definite
distribution of the protuberances on the solar surface, ha
ally generalized from the conditions then verified, and hav
accused the results deduced by me at a former period of
having done this chiefly with regard to that of the abs
protuberances in the polar regions.

By a fortunate combination, the regular observations of
rances were commenced by me some time before the dev
that condition of extraordinary activity from which the r
the spots is produced ; and they were subsequently protra
that abnormal condition of the sun, and also after it.

There could have been no better circumstances dur
period of observation, for instituting a valid comparison l
two solar phenomena of the spots and the protuberanc
observing the relations existing between them ; and
obtained in this respect by my observations on the pr
may, I think, be accounted as the most important dat
procured, concerning the activity and physical constitu
sun.

From the tables which I inserted in my Note IV., the
increase in the number and in the size of the protubera
clearly demonstrated, and their condensation in the pola
correspondence with the greater development of the spo
ascertained, as to render it certain that on the diminution
solar activity also (as manifested in the protuberances c
would be reduced to the smallest proportions, and con

equatorial regions, restoring to the polar regions perfect freedom from those eruptions which had broken out in them during the abnormal period of extraordinary perturbation.

These consequences could not be deduced from the observations made during the first period of the maximum of the spots; and thus, while the result obtained by me during my first observations (namely, of the large protuberances in the polar regions) is declared to be inaccurate, their progressive condensation towards the poles in the second period of observation is also denied.

But the observations made this year, in which the sun is found to be reduced to a condition of more normal activity, have fully justified my conclusions; since, the protuberances decreasing sensibly in number and size,—the polar regions are shown to be again free from large protuberances, and almost with greater constancy, and over a larger extent than that found in the first period of observation.

In confirmation of this, I will give some statistical data obtained from my observations.

Although the number of the protuberances observed on each day on the outline of the sun cannot be reckoned as in proportion to the number of the protuberances really existing on the surface of it; and although even the average of the diurnal numbers of the protuberances, observed by me through a long series of days, cannot be held as strictly in proportion to the constant average of the protuberances on the surface of the sun in the same space of time, yet these data are sufficient to furnish us with a just idea of the greater or less frequency of the phenomenon, and thence of the state of greater or less activity of the solar surface.

Wishing, then, to study the frequency of the phenomenon in the several solar zones, taken in relation to the equator or the poles, we meet with a grave difficulty, namely, that of determining the latitudes of the protuberances; the angle of position of the point of the solar outline on which they are projected, not being sufficient datum, their base, especially for the large protuberances, being rather far from that spot, and in a very different parallel, especially when in the vicinity of the poles.

For this reason, I considered it useless to undertake so large a task as that of calculating for the apparent angle of position, the latitudes of the thousands of protuberances observed; and I confined myself to deducing these latitudes from the difference between the angle of position of the prospective projection of the polar axis on the outline, and the angle of position of the protuberances.

This process is indeed too imperfect and inaccurate, especially for the protuberances near to the poles; but the influences of the errors become less sensible, giving to the zones a notable extension, at least of 10°; and by each method we can obtain data sufficiently approximate to furnish a sure idea of the greater or less frequency of the phenomenon, considered separately in every zone, in correspondence with the various periods of observation.

To obtain the variations arrived at in the frequency and in the size of the protuberances in the various solar zones, I have divided the entire period of observation into five partial periods, indicated above, in which we get the following numbers of complete profiles:—

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1st Period from Oct. 1869 to April 1870, with 71 pro
2nd „ „ May 1870 „ Oct. 1870, „ 79
3rd „ „ Nov. 1870 „ April 1871, „ 31
4th „ „ May 1871 „ Sept. 1871, „ 67
5th „ „ Jan. 1872 „ April 1872, „ 37

Although this report of the observations does not sho
formity with regard to the number of the days of obser
I thought it desirable to adopt it for the utilization of
servations made ; not, however, with the intention of of
which would make known the true proportion accordin
the frequency of the protuberances during the various
varied, but data sufficient to ascertain the direction of the v
that is, sufficient to prove whether the frequency itself in
indicated, was in the direction of increase or of decrease.

In the following Table the total numbers of the pro
observed in each of the five periods taken, and in the several
from 0° to 10° of polar distance, are reported ; but in thes
the small jets and the little risings of the chromosphe
represented, while the complex protuberances are represent
divided into the principal jets of which they are composed.

TABLE I.

TOTAL NUMBER OF THE PROTUBERANCES.

Polar Distances.	NORTHERN HEMISPHERE.					SOUTHERN HE		
	Periods.					Period		
	I.	II.	III.	IV.	V.	I.	II.	III.
0′ to 10′ . . .	26	50	19	147	34	14	24	11
10 „ 20	35	102	38	163	41	33	32	23
20 „ 30	94	175	30	72	37	76	86	42
30 „ 40	93	86	21	104	60	91	110	10
40 „ 50	115	116	23	148	81	110	119	22
50 „ 60	103	126	52	147	96	98	123	54
60 „ 70	116	138	48	145	90	135	140	51
70 „ 80	92	146	41	137	74	95	123	59
80 „ 90	68	118	31	132	72	72	125	37
Northern Hemisphere. . .	742	1058	303	1195	585	724	882	309
Southern Hemisphere. . .	724	882	309	1218	588			
Total	1,466	1,940	612	2,413	1,173			

From this Table we find that the frequency of the pro
varying sensibly from one period to the other, is neverth
tained in sensible equality in the two hemispheres, except in
period, during which the number is sensibly greater in th
hemisphere. The total number of the protuberances c
the 285 solar profiles is 7,604, of which 3,883 are in the nort
sphere and 3,721 in the southern hemisphere. To render

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the comparison of the frequency of the protuberances in the various periods, either in the total outline of the sun, or in a single hemisphere, and thus in single zones, the following Table has been prepared ; it contains the diurnal averages of the protuberances observed on each day in every zone, and they were obtained by dividing the numbers given from Table I. by the numbers of the corresponding solar profiles.

TABLE II.

DIURNAL NUMBER OF THE PROTUBERANCES.

Polar Distances.	Northern Hemisphere.					Southern Hemisphere.				
	Periods.					Periods.				
	I.	II.	III.	IV.	V.	I.	II.	III.	IV.	V.
0′ to 10′ . . .	0·37	0·63	0·61	2·19	0·92	0·20	0·30	0·36	1·98	1·43
10 ,, 20	0·49	1·29	1·23	2·43	1·11	0·46	0·41	0·76	2·92	1·16
20 ,, 30	1·32	2·22	0·96	1·07	1·00	1·07	1·09	1·36	0·90	1·24
30 ,, 40	1·31	1·09	0·68	1·55	1·62	1·28	1·39	0·32	0·91	1·05
40 ,, 50	1·62	1·47	0·74	2·21	2·19	1·55	1·51	0·71	1·91	1·57
50 ,, 60	1·45	1·59	1·68	2·19	2·59	1·38	1·56	1·74	2·33	2·35
60 ,, 70	1·63	1·75	1·52	2·16	2·43	1·90	1·77	1·65	2·61	2·54
70 ,, 80	1·29	1·85	1·33	2·04	2·00	1·34	1·56	1·90	2·54	2·35
80 ,, 90	0·96	1·49	1·00	1·97	1·94	1·02	1·58	1·19	2·06	2·19
Northern Hemisphere. . .	10·44	13·38	9·75	17·81	15·80	10·20	11·17	9·99	18·16	15·88
Southern Hemisphere. . .	10·20	11·17	9·99	18·16	15·88					
Total	20·64	24·55	19·74	35·97	31·68					

From this Table it is seen that from the first to the second period, the number of the protuberances is notably on the increase ; that, stopping at the third period, they are developed in larger proportions in the fourth, in which the number of protuberances becomes almost double that of the first and of the third ; while in the last period a sensible decrease is presented.

These variations appear simultaneously in the same direction in the two hemispheres, but not in the same proportions. In the various zones these variations are rather at variance in the succeeding periods ; since while in some zones the frequency of the protuberances rapidly increases, in others, on the contrary, they are apparently stationary or decreasing.

To render these variations in the several solar zones more clear, and for the better recognition of the direction and proportions in the several periods, I have thought it desirable to give in another Table the graphic construction of Table II., representing the numbers of the protuberances, corresponding to each zone, with the ordinates of a curve the abscissiæ of which indicate the zone.

From this Table are deduced : –

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1. That in the first period, the protuberances were alm
trically distributed in the two hemispheres, with a minimum
in the equatorial zone included between the parallels of 80° fr
pole and 80° from the south pole, with two minima sufficie
at the flattened tops of the poles, and with two scarce
maxima in the zones included between 60° and 70° north ar

2. That in the second period, the frequency of the pr
with a scarcely sensible minimum, is almost constant fro
polar distance to 40° south polar distance, two very mar
in the polar regions, and a very marked maximum in t
hemisphere, in the zone included between 20° and 30° pola

3. The curve is rendered more complete in the third pe
which we have, besides the equatorial minimum and the
also the other two minima—one between 30° and 40° no
other between 70° and 80° south; and two other seconda
marked maxima—one between 10° and 20° north, anc
between 20° and 30° south.

4. In Period IV. the frequency of the protuberance
constant from 40° north to 80° south polar distance, with
between 60° and 70° south, with two absolute minima betw
30° north and south, two very decided maxima between
north and south, and two scarcely indicated minima at the

5. Finally, in Period V. the frequency of the protuberar
in the several zones, a progress (advance?) which res
verified in Period I. by the equatorial minimum, with two n
between 50° and 60° north, and the other between 60° an
with a notable and almost constant depression in the flatte
the poles included between the parallels of 30° north and 4

These partial results, considered as a whole, show in
that from Period I. to Period IV. the solar activity,
by the frequency of the protuberances, became some
intense, especially towards the polar regions, in which
noticeably weaker in Period V., during which it is ten
festly to resume that more normal condition in which it
tained to be in the first period of observation, that is,
maximum of the solar spots. This result is of the utn
tance, because, not only does it afford the greatest evide
close elation and dependence of the two solar phenom
berances and spots), but more than this, it shows that in corr
with the maximum of the spots, not only the zones subject t
in an abnormal state of activity, but also that the polar regi
pated in the same state.

This fact is rendered still more manifest, considering
activity as shown in the frequency of the large protubera
various zones; since it is found that, in correspondenc
maximum of the spots, not only does the number of the
tuberances increase, but more than this—that their distrib
the solar surface is radically modified.

Choosing those protuberances higher than three terrestria
or not less than 1', I have compiled the following table (Ta
which the number of those protuberances in each of the
above fixed is given for every solar zone.

TABLE III.

Total Number of the Large Protuberances.

Polar Distances.	Northern Hemisphere.					Southern Hemisphere.				
	Periods.					Periods.				
	I.	II.	III.	IV.	V.	I.	II.	III.	IV.	V.
0° to 10° . . .	0	0	11	21	2	0	0	1	26	1
10 ,, 20	0	21	16	40	0	0	0	2	61	0
20 ,, 30	8	44	8	8	0	12	16	4	10	0
30 ,, 40	16	15	2	27	5	25	32	7	7	2
40 ,, 50	18	23	4	39	17	25	25	7	30	5
50 ,, 60	15	24	14	38	18	9	23	16	52	19
60 ,, 70	12	25	12	34	14	18	39	8	39	15
70 ,, 80	7	35	8	21	11	7	20	12	32	17
80 ,, 90	6	13	11	16	10	8	17	13	19	9
Northern Hemisphere . . .	82	200	86	244	77	104	172	70	276	68
Southern Hemisphere . .	104	172	70	276	68					
Total	186	372	156	520	145					

TABLE IV.

Diurnal Number of the Large Protuberances.

Polar Distances.	Northern Hemisphere.					Southern Hemisphere.				
	Periods.					Periods.				
	I.	II.	III.	IV.	V.	I.	II.	III.	IV.	V.
0° to 10° . . .	0·00	0·00	0·36	0·31	0·05	0·00	0·00	0·03	0·39	0·03
10 ,, 20	0·00	0·27	0·52	0·61	0·00	0·00	0·00	0·06	0·94	0·00
20 ,, 30	0·11	0·56	0·26	0·12	0·00	0·17	0·20	0·13	0·15	0·00
30 ,, 40	0·23	0·19	0·06	0·40	0·14	0.35	0·41	0·23	0·10	0·05
40 ,, 50	0·25	0·29	0·13	0·61	0·46	0·35	0·32	0·23	0·45	0·14
50 ,, 60	0·21	0·30	0·45	0·58	0·51	0·13	0·29	0·52	0·81	0·54
60 ,, 70	0·17	0·32	0·39	0·52	0·38	0·25	0·49	0·26	0·60	0·41
70 ,, 80	0·10	0·44	0·26	0·31	0·30	0·08	0·25	0·39	0·48	0·46
80 ,, 90	0·08	0·16	0·32	0·24	0·27	0·13	0·22	0·42	0·28	0·24
Northern Hemisphere . .	1·15	2·53	2·75	3·70	2·11	1·46	2·18	2·27	4·20	1·87
Southern Hemisphere . .	1·46	2·18	2·27	4·20	1·87					
Total	2·61	4·71	5·02	7·90	3·98					

The total number of the large protuberances contained in the 285 profiles is 1,379, namely, 689 in the northern hemisphere and 690 in

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the southern hemisphere. Although in average result, ther
the same number of large protuberances in the two hemisph
in each separate period there are sensible differences, from w
deduced, that the frequency of the large protuberances was al
prevalent in one of the two hemispheres. This shows that
activity does not prevail with the same energy in the two hen
at the same time; but that the temporary differences durin
period are almost totally compensated.

To render more clear the law by which the frequency in
protuberances has varied, whether in the entire surface of
or in its hemispheres, or, lastly, in its different zones, I have
the foregoing table (Table IV.), which gives the relative fre
the phenomenon in the different zones, deduced from the
given by the solar profiles corresponding to each period.

From this table it is seen that the frequency of the larger
rances is progressively increased from the first to the fourt
diminishing sensibly in the fifth: and that from the first to t
period the number of the large protuberances is tripled. Thi
ical variation is presented in both hemispheres; but in pr
sensibly different. In the northern hemisphere, the increa
large protuberances is produced with greater regularity tha
southern hemisphere, in which the rapid increase observed
third to the fourth period is chiefly remarkable.

With the intention of making clearer the variations of freq
the different solar zones, I have given in another Table the gra
struction of this last table. Examining this Table, the
results are deduced:—

1. In the first period of observation the large protuberan
scarce at the equator, in the zone included between 70° polar
north and 70° south: in the northern hemisphere, the maximu
large protuberances was in the zone between 40° and 50°, a
southern hemisphere equally between 40° and 50°; while in
regions, for a distance from the poles of about 20°, the large
rances were totally absent.

2. In the second period there was a minimum in the e
zone: in the northern hemisphere, a maximum between 70°
polar distance, a second minimum between 30° and 40°, a
developed maximum in the zone from 20° to 30°, with total a
protuberances in the polar regions from 0° to 10°: in the
hemisphere there was a maximum between 60° and 70°, a
between 50° and 60°, with a second maximum slightly d
between 30° and 40°, while the large protuberances were totall
in the polar regions from 0° to 20°.

3. In the third period, the equatorial minimum having disa
there was in the northern hemisphere a maximum between 50°
with a decided minimum between 30° and 40°, followed by a
developed maximum between 10° and 20°, with notable freq
large protuberances, also in the polar regions: in the southe
sphere there was, on the other hand, a maximum between 80°
a minimum between 60° and 70°, a very decided maximum bet
and 60°, with progressive and rapid diminution, and alm
absence of large protuberances towards the pole.

4. In the fourth period, there was a scarcity of protuberances at the equator, in the zone included between 70° north polar distance and 70° south: in the northern hemisphere, a first maximum from 40° to 50°, followed by a tolerably decided minimum between 20° and 30°, with a second maximum between 10° and 20°, with sensible diminution at the pole; in the southern hemisphere, on the other hand, a very developed maximum between 50° and 60°, followed by a very marked minimum between 30° and 40°, and by an extraordinary maximum between 10° and 20°, with sensible decrease towards the pole.

5. Finally, in the last period of observation, a less frequency of protuberances continues in the equatorial zones: in the northern hemisphere, there is a maximum between 30° and 60°, with an almost absolute absence of protuberances in the polar regions until 30°: in the southern hemisphere, there is a maximum between 30° and 60°, after which the large protuberances disappear almost totally up to the pole.

The examination of the curves representing the frequency in relation to the large protuberances in the different solar zones through five periods, clearly shows that by the progressive increase of the solar spots, the large protuberances are not only rendered more frequent on the zones of the spots themselves, but also to great distances, even up to the poles, where generally the conditions necessary to their development are wanting.

This fact, then, comes as an evidence that the period of the spots does not depend on a partial modification of the condition of the surface or of the solar body in the zones where the spots prevail, but from a very much more general condition, namely, that of great activity on the total surface, and perhaps in the entire solar body: and of this very important datum we must not lose sight in the investigations concerning the causes of the period of the solar spots.

For the present, we cannot predict whether this datum will render such a question more simple or more mysterious; but it is certain that any theory on the period of the spots must remain defective, if it does not also give a reason for this phenomenon, that is, for the periodical variations in the frequency, size, and positions of the protuberances.

In my Note IV. of July 30, 1871, although it could be based only on the observations made before the maximum of the solar spots and during it, yet the progressive advance of the protuberances towards the poles, in correspondence with the greater development of the spots, induced me to admit such a phenomenon, as a great exception to the law deduced from the first observations, of the absence at the poles of the large protuberances,[1] in the full conviction that, this critical period having ceased, the protuberances would again emigrate from the polar regions.

The observations of the last period have completely justified this conclusion, and I am fully persuaded that they who (founding their assertions on their observations confined to the exceptional periods) have pronounced as inaccurate the law relating to the absence of the

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[1] From the time of the reading of this Note, until the end of June, the observations were continued regularly, and during these last two months I never found protuberances at a polar distance less than 30°, so that for about five months the polar regions have been free from such protuberances.

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large protuberances at the poles in the normal conditions o
and that of their condensation at the poles in the critical
the maximum of the spots, will by their subsequent observa
fully recognized the truth of those laws which, in my opini
tute the most important results of the observations made
phenomena. What seems to me deserving of great considerati
while in the frequency of the spots there occurred during
of observation some irregularities, such as to render it very
fix the exact period of the maximum, in the frequency of
berances, and in their successive development in the polar
much more decided progress is ascertained, by which the ep
greatest perturbation is manifestly confined to the summ
of 1871.

This seems to prove that the period of the greatest sol
cannot be deduced from that of the greatest frequency of th
the solar disc, but rather from the epoch of their most ra
formation, of their most rapid appearance and disappearanc

The period of observation was not certainly extended enc
able to establish the definite laws of the periodical approa
distribution of the protuberances on the solar surface in
dence with that of the spots, but the data obtained pr
already the existence of those laws, to discover which is no
than a question of time and patience.

Before concluding this article relative to the distribution
tuberances over the solar superficies, I think fit to say somet
especial characteristic noticed in the distribution of the
illustrious Father Secchi. He remarked on the circumsta
frequent coupling of the large protuberances at the extrem
and the same solar diameter, a fact which in his opinion mu
origin in the solar structure, which must be of such a nature
these alternations to be propagated from one end to the other
body, that such effects are produced, from which may be c
(although by no means to be announced as a proved fact) th
is constituted of an elastic fluid.

In treating of such a fact, I thought proper to seek fo
firmation of it in the completion of my observations, n
accurate computation of the cases favourable to this coincid

From this computation it has resulted that, considering
able even those cases in which two large protuberances
at the extremity of one and the same solar diameter within
limits of 5° or 6°, the number of the coincidences is more lin
would be anticipated from the accidental combinations,
hypothesis that the eruptions or protuberances are produce
in the different parts of the solar surface. And I maintai
reason of this scarcity of coincidences must be attributed
cumstance that the solar activity generally is presented
different intensity in the two hemispheres, and especially
sponding zones with regard to the equator. In consequen
while such coincidences must be regarded as simply acci
must consider as wholly without foundation the conjectur
been deduced from them, with regard to the physical and
conditions of the solar mass.

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